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G. B. AIRY, Esq., President, in the Chair.

John Ashton Nicholls, Esq., Ardwick Place, Manchester; and James Nasmyth, Esq., Patricroft, near Manchester; were balloted for and duly elected Fellows of the Society.

The Astronomer Royal resigned the Chair to the Rev. R. Sheepshanks, and then delivered to the Society an oral statement, illustrated by models, "On Instruments adapted to the Measure of small Meridional Zenith Distances."

The Astronomer Royal remarked that there were three distinct ranges of observation, for which three distinct classes of instruments had been employed. The first and most comprehensive included the observations for zenith distances throughout the whole extent of the meridian, from the north horizon to the south horizon: to which observations the mural circle and similar instruments were adapted. It was evident that constructions adapted to secure this extent of observations in all their generality could scarcely be expected to avail themselves of the advantages peculiar to observations confined to one part of the heavens; and accordingly, in nearly every instance in which observations on a more limited arc of the meridian were sufficient, instruments of the other classes had been employed. The second class of instruments might be included under the name of zenith sector. With this instrument, by limiting the range of observation to six or seven degrees on each side of the zenith, a part of the arc of meridian was embraced which sufficed for most geodetical purposes; that part, both from the smaller proportion of obscuration by clouds and from the smaller uncertainty attending the calculation of refraction, was better in an astronomical point of view than the rest; the instrument also might, with great portability, be made of large dimensions, with long telescope and long plumb-line, two advantages which were highly valued in the last and in the beginning of the present centuries. Generally, its use had been confined to geodesy; although it must not be forgotten

that one of the most celebrated specimens of it, namely, Bradley's zenith sector, was constructed solely for astronomical observations at a fixed observatory; still, however, that instrument had in practice been confined to the observation of a single star passing the meridian within two minutes of the zenith, and it might thus be considered as included in the third class. The third class included those instruments specially adapted to the observation of stars passing within still smaller distances of the zenith. It is evident that these, besides possessing in the highest excellence the astronomical advantages ascribed to the second class, might be expected to enjoy extraordinary freedom from the distortions produced by the weight of the instrument, and might also give great facility for measures of zenith distance by the simple use of the micrometer or the spirit-level. This was the class of instruments which the Astronomer Royal proposed as the subject of the present statement.

The Astronomer Royal announced that his discussion would be confined to a critical description of three instruments: the Greenwich 25-feet Zenith Tube, now dismounted; Struve's Prime Vertical Instrument, now in use; and a construction of a Reflex Zenith Telescope, proposed by himself, but not yet actually constructed. The second of these instruments, it is true, may be used for zenith distances of several degrees, but its peculiar advantages are connected with its use for very small zenith distances. These three instruments would be found to embody, as essential parts of their construction, the three different methods of referring to the zenith in use in modern times, namely, the plumb-line, the spirit-level, and the reflexion from the surface of quicksilver.

I. The Greenwich Zenith Tube was planned and constructed by Troughton, several years before Mr. Pond's direction of the Royal Observatory ceased; although, from some delays in regard to the small but essential parts, it was not brought into use till within two years of that time. It consisted of a telescope 25 feet long, revolving in azimuth on a pin at the bottom and between guides near the top, but absolutely confined to the vertical direction. Its range of observation was therefore necessarily limited to that angle in which the injurious effect of obliquity of the pencil upon the image of the star is insensible; and practically it was confined to the single star γ *Draconis*, at two minutes only from the zenith. The aperture of the object-glass was 5 inches; the diameter of the tube at the top 6 inches, which increased at each successive step downwards (the tube being made in five separate lengths), till it was 10 inches in diameter at the bottom. About 6 inches below the object-glass, on the outside of the tube, was a reel of silver wire; the wire passed upwards over a wheel admitting of a certain degree of end-motion, and then passed through a hole in the side of the tube, and rested between the threads of a screw, within the tube and 4 inches from its top, whose axis was horizontal (a convenient arrangement for small movements of the point of suspension), and then descended vertically. Within the great tube was a small vertical tube, nearly

an inch in diameter; the top of this tube was about 10 inches below the object-glass, and here the small tube was in contact with the side of the great tube; the tube extended to the lower end, at which part it was separated about 2 inches from the side of the great tube. The plumb-line descended through this vertical tube, and passed through a hole in the bottom plate of the telescope, and there it carried the plumb-bob in a cup of water supported by hooks upon the lower plate. Just above the top of the small tube, or 9 inches below the top of the great tube, and consequently 5 inches below the point of free suspension of the wire, a micrometer-microscope was fixed externally to the tube, on the same side on which the wire tube is placed, for reading the position of the wire. Very close to the bottom of the tube, but on the side opposite to that on which the wire tube is placed, another micrometer-microscope was placed for reading the position of the lower part of the wire; the wire tube here was perforated to permit vision of the wire. This microscope was almost completely buried in the great tube (its micrometer and eye-piece alone projecting from it); it was not, however, firmly screwed down; it was thrust in by hand into a hole (where it was held by friction only) in a sliding-plate within the great tube, which sliding-plate was moved by external screws. It is evident that, if these microscopes were firm, and if the wire retained in all positions of the instrument its peculiarities of curvature, &c. in constant relation to the *instrument*, while its general direction of dependence had respect to nothing but the *direction of gravity*, the readings of the micrometer-microscope would give most accurate information upon the position of the great tube with respect to the vertical (the observation of the sides of the plumb-wire being exceedingly delicate). Below the lower plate of the great tube was the eye-piece of the telescope, a 4-glass diagonal eye-piece, its eye-end projecting horizontally below the eye-piece of the lower micrometer-microscope, and nearly to the same distance. With this was viewed the wire (carried by a micrometer, to which was given for distinction the name of Grand Micrometer), by which the bisection of the star was effected. The lower plate of the telescope was supported by four arched pieces of brass, attached to the lower plate at its circumference, and united about 6 inches below its centre; and at the place of union was the pin which turned in the foot-plate. The intervals between the brass arches left room for the eye-piece, the micrometer, and the plumb-line cup. The foot-plates had adjusting screws, which were, however, very rarely used. The upper guides of the great tube were about 5 feet below the top; they were carried by an enormous iron tube, which inclosed the great tube to within about 4 feet of the bottom, and was there supported by large iron arches rising from four large iron pillars, which at some distance surrounded the pier of the foot-plates, being placed at the angles of a square, and leaving a clear space of $4\frac{1}{2}$ feet between each pillar and that opposite to it.

When the present Astronomer Royal took charge of the Royal

Observatory. he found the Grand Micrometer of this instrument in the following state :—The wire-plate of the micrometer carried also the long heavy eye-piece projecting sideways more than 6 inches; the long micrometer-screw extended nearly from side to side of the lower plate of the tube, was supported at its extremities by projections from that lower plate, and was tapped through the case of the diagonal mirror, thus acting at a distance of about 1 inch from the wire-plate. The supports of the ends of the screw, upon whose firmness the correctness of the action of the screw must entirely depend, were thin vertical arches attached to the bottom plate only by small thumb-screws, and these arches sustained the force of the micrometer-screw sideways. Three arrangements more entirely opposed to the just principles of construction of micrometers can scarcely be conceived. The wire-plate ought not to be strained sideways by a projecting weight, and ought not to carry any weight which will increase the friction of its movement; the action of the screw ought to be in the plane of motion of the wire-plate; and the supporting point, or *appui*, of the screw ought to be perfectly firm.

The following changes were therefore made without delay. The long screw was laid aside, and in its place was used a micrometer-screw or stalk attached to the end of the wire-plate in the usual way; and the micrometer was made in every respect like an ordinary micrometer, the micrometer-head carrying the concave-screw which embraces the stalk and draws it towards the micrometer-head, the action of the screw being in the plane of the wire-plate and directed longitudinally across the middle of the wire, and the drawing action of the screw being resisted by springs at the opposite end of the wire-plate. The motion of the micrometer was limited in this construction to about one-tenth of the whole breadth of the end-plate of the telescope, but this was far more than was required to measure the zenith distance of γ *Draconis;* it was, however, made subservient to the measure of nearly the whole breadth in the following way :—On the fixed plate of the micrometer ten crosses of wires were fastened, as nearly as possible at equal distances, and on the moveable plate were fixed eleven wires at nearly the same distances. By means of the crosses on the fixed plate and the micrometer-movement, the interval from each wire on the moveable plate to the next wire was ascertained, in terms of the micrometer-screw, with great accuracy, and therefore the aggregate or distance of extreme wires was very well known. The intervals were also known in seconds of arc, by observations of the transits of stars when the instrument was turned to a position distant 90° in azimuth from its usual position. Thus the value of the screw, and the elements for making an observation in any part of the whole range available, were completely obtained. With this construction of micrometer, the *appui* of the micrometer-head was almost close to the bottom plate, and was perfectly firm. The eye-piece was carried by another sliding-plate, moving in grooves unconnected with the micrometer; and it was moved by a separate

rack and pinion. It is presumed that in this form the Grand Micrometer was perfectly trustworthy.

In the use of this instrument it is evidently necessary to observe the same star successively in reversed positions of the instrument, the micrometer-head being on the north side in one observation and on the south side in the other observation; and the values of the Grand-Micrometer reading must be corrected in each observation by quantities depending on the two micrometer-readings of the plumb-line, in order to obtain for the two observations the angular distances of the star from definite (though imaginary) lines in space, equally inclined to the vertical, one on the north side, the other on the south side; and if this double operation be effected at one transit of the star, the result for the star's zenith distance is obtained without any computation of the star's corrections, and without any reliance on the permanence of the state of the instrument for more than a few minutes; but if the observation with micrometer-head north is made at the transit of one day, and that with micrometer-head south at the transit of another day, it is necessary to compute the change in the star's place (which can be done with undoubted accuracy), and also to be assured that no change has taken place in the relative position of the various parts of the instrument. For the details of the calculations of every kind applicable to these cases, it is best to refer to the Introductions to the volumes of *Greenwich Observations* from 1837.

For a considerable time, the instrument, not being furnished with two wires on the micrometer-plate at a distance nearly equal to double the star's zenith distance, could not be used for the double observation at a single transit (as the short duration in the field of view did not leave time enough for the numerous turns of the micrometer, with change of the observer's position, &c.), and it was therefore reversed after the completion of each day's observation. The results of the observations were not satisfactory, and the Astronomer Royal determined on so fitting up the instrument that it might be used for the double observation at a single transit.

The arrangements obviously necessary were, to fix on the micrometer-plate two wires at distances nearly equal to double the star's zenith distance, and to determine their interval accurately; and to provide stops for the reversion of the instrument, for the movement of the eye-piece, and for the movement of a part of the illuminating apparatus, which it was necessary to shift at each observation. But another change, the necessity for which seems to have been generally overlooked, then suggested itself. It is unsafe to consider any wire as absolutely straight: when it has once received a bend, even no more than the bend of passing round a reel, it retains a portion of that bend although stretched even to the breaking-point; and some of those curved parts of the wire may be under the microscopes by which the position of the plumb-line is checked. Now this is unimportant, provided that the bend is always turned in the same direction relatively to the instrument;

for then its only effect is to alter by a constant quantity the correction to the value of the Grand-Micrometer reading, which constant disappears on taking the difference of the two corrected Grand-Micrometer readings, upon which in fact the determined zenith distance depends. But if the position of the bend changes relatively to the instrument, the result is affected with error. Now it is extremely probable that the position of the bend will change, that is, that the plumb-line will turn relatively to the instrument, at every reversion, but more particularly so at rapid reversion; and some method must be adopted to prevent this turning.

For this purpose, the Astronomer Royal adopted the following construction: — On the side opposite to the eye-piece, and attached to the bottom plate of the telescope, a wooden box was carried out horizontally, its bottom being nearly as low as the foot plate, and its top at the level of the bottom plate of the tube; and upon the end of this box was planted a wooden plumb-wire tube, connected at its top with a frame attached to a higher part of the great tube (upon which frame the wooden plumb-wire tube and the horizontal box were, in fact, suspended). From the top of this wooden tube was suspended (with screw-adjustment for moving it to or from the principal plumb-line) a second plumb-line, 40 inches long, and distant from the principal plumb-line about 12 inches. The two plumb-lines supported in the horizontal box the two ends of a bar 12 inches long, and to this bar were attached one or two plumb-bobs. A little consideration of the theory of parallel forces will shew that, if the distance between the plumb-lines at the top and the bottom is the same (the criterion of which is, the distinctness of the principal plumb-line in the field of view of the lower microscope, whether the second plumb-line be used or not), the relative movements of the principal plumb-line for varying inclinations of the grand tube are the same as if the plumb-bob were immediately attached to it. At the same time, the turning of the wire is effectually prevented.

The instrument was used with this construction to the spring of 1848, when it was finally dismounted. The results of observation were more accordant than they were before introducing the last modification, but they were not superior to those derived from the mural circle. There can be no doubt that the remaining errors were greater than could be attributed to mere imperfection of observation, and that they must originate in some fault of the instrument. Generally speaking, the greatest errors coincided with the greatest irregularities in the readings of the lower plumb-line microscope. It has already been pointed out that the fixation of that microscope was not very firm, and the irregularities may have originated in this weakness. Or the great tube may have twisted sensibly in the reversion. Or the plumb-line may have been bound by spider-threads, from which it was to a certain degree set free by the movement of reversion (for it was found almost impossible to keep the wire-plate free from spider-threads, the uniform temperature and the constantly vertical position of the telescope being probably com-

fortable to those animals). Whatever the distinct cause might be, the Astronomer Royal considered that the instrument had failed, and that its failure was owing to the dependence on the plumb-line; and he expressed his hope that he might never again be compelled to use an instrument relying for its verification upon a plumb-line.

II. The applicability of a transit instrument in the prime vertical to the determination of latitude of place from assumed polar distance of the star, or *vice versâ* (the polar distance of the star being greater than the co-latitude of the place), has long been known; but it seems to have attracted more particular attention since it was used by Struve in the determination of the difference of latitudes at the extremities of his arc of meridian parallel to the Baltic. Whatever be the construction of the transit instrument used, the proper method of observation is the following:—The error of level of the axis being ascertained, the instrument is directed to the star, while it is yet north of the eastern prime vertical, and the transit of the star is observed over each of the wires preceding the middle of the field; the position of the instrument being continually changed, so that the oblique transit is observed over the centre of each wire. When the star has passed the wire next before the middle, the instrument is reversed, and the passage of the star, now on the south side of the eastern prime vertical, is observed over the same wires as before, but in the opposite order. The error of level of the axis is then ascertained. Then, when the star is approaching the western prime vertical from the south, the instrument being still in its second position, the error of level of the axis is again ascertained; then the transit of the star is observed again over the same wires; before it passes the middle of the field, the instrument is reversed to its first position; then the transit of the star, now on the north side of the west prime vertical, is observed again over the same wires. Finally, the error of level of the axis is ascertained in this position.

The reduction of the observations is made in the following form, each wire being treated separately. The projection of each wire on the sky is a small circle whose pole is in the north point N of the horizon: and if α be the angular distance of that wire from the line of collimation, $90^\circ - \alpha$ will be the radius of the small circle when the star is seen on it north of the prime vertical, and $90^\circ + \alpha$ when the star is south of the prime vertical. Form the spherical triangle NPS; let A be the hour-angle from the meridian, or the supplement of the angle at P; l the latitude of place; δ the star's north polar distance. Then when the star is north of the prime vertical,

$$\cos(90^\circ - \alpha) = \cos l \,.\, \cos\delta - \sin l \,.\, \sin\delta \,.\, \cos A_1$$

and when the star is south of the prime vertical,

$$\cos(90^\circ + \alpha) = \cos l \,.\, \cos\delta - \sin l \,.\, \sin\delta \,.\, \cos A_2$$

Adding these two equations, we obtain

$$\cot l \,.\, \cot\delta = \cos\frac{A_1 + A_2}{2} \cdot \cos\frac{A_1 - A_2}{2}$$

A_1 is half the interval between the first transit east and the second transit west, and A_2 is half the interval between the second transit east and the first transit west, in both cases converted into arc. Thus by the observations on each wire we determine with great facility l from δ, or δ from l, without any knowledge of the distance of that wire from the line of collimation.

The mean error of level of the instrument may be applied to l before forming the logarithm of cot l (supposing that it is the object of the observation to determine δ). Or a correction may be applied for it (which will be different for different stars) to the result obtained with a constant value of l. The latter is the course followed by Mr. Struve in reducing the observations of which I shall shortly speak.

The advantages peculiar to this observation are; that it is not affected by ordinary refraction; and that its scale, being one of time, is exact to a degree which is unapproachable in other ways. Bad, indeed, must the clock be of whose rate we are not certain within 1^s per diem; therefore the uncertainty on our time-scale cannot practically amount to $\frac{1}{80000}$ part of the whole, and that on the results of the observation, as depends on this cause, cannot amount to $\frac{1}{40000}$ part of the whole. But the Astronomer Royal stated, as the result of his own experience, that an accuracy of $\frac{1}{1000}$ in the determination of a micrometer-scale is almost more than can be hoped for. Other advantages which it possesses are common to other reversible instruments. Thus if there be a constant optical fault in the image of a star, produced by defects in the object-glass, that fault will produce opposite effects in the first and second positions of the instrument: if the pivots be mis-shapen, as if there be a hump upon one, yet if the form of the two Y's is similar, that hump will take the same bearing upon the east side of one Y in one observation as upon the west side of the other Y in another observation, and its effects will be annihilated in the result: if the pivots are unequal, the effects of the inequality are similarly annihilated. But to secure all these advantages, the two following instrumental points must be secured in the construction; the instrument must admit of having the level applied to it while the telescope is in the position of observation; and it must admit of being reversed with ease and rapidity. To the obtaining of these objects, the construction devised by Mr. Struve, and carried out by Messrs. Repsold, is particularly well adapted. This instrument is supported upon Y's carried by two stone pillars, about 6 feet high and 46 inches apart, from outside to outside; the *outside* face of the pillars being vertical, and the *inside* faces inclined to the vertical. The axis of the instrument has its bearings upon the two Y's; but the telescope (7 feet long) attached to this axis is *on the outside of one of the pillars;* a counterpoise at the other end of the axis being on the outside of the other pillar. Between the two pillars is the reversing apparatus, which also carries the ordinary counterpoises. It consists of a vertical shaft, sliding through holes in cross-bars which are fixed to the piers, and prevented by

a fillet upon it from turning until it is raised to a certain height; this vertical shaft carries a T head about 33 inches long, at the extremities of which are the lifting-forks, and also the fulcra of the ordinary counterpoises. The counterpoises act by means of levers to support a bar about 41 inches long, at the extremities of which are the friction-rollers, which at all times support the principal part of the weight of the instrument. The vertical shaft does, therefore, at all times support the fulcra-pressures of the counterpoises; and when the instrument is raised for reversion, by bringing up the vertical shaft so that the lifting-forks at the ends of the T head come in contact with the axis of the instrument, the only additional load which is put upon the vertical shaft is that pressure which was left as residual weight upon the Y's on the stone pillars, a pressure which, in the practice of the German astronomers, is very small. Two massive lever-counterpoises are therefore provided below, which act upwards under the foot of the shaft: and if these are adjusted to support the shaft when the instrument is not on the lifting-forks, they will also practically nearly support it when the instrument is on the lifting-forks: so that a very trifling effort of the hand is then necessary to raise the shaft with the instrument. This small effort is given through a winch, acting by means of bevel-gearing upon a large circular nut, which works on a screw-thread cut upon the shaft, and bears vertically upon one of the cross-bars: and thus by an exertion which appears almost fancifully small, the instrument is raised for reversion, is turned round (the telescope being placed horizontally), and is deposited in its new position. Of this reversing apparatus the Astronomer Royal spoke with great praise. Not only is the reversion effected with a rapidity and ease scarcely to be conceived, but also the counterpoises are acting in the same way, and the general strains upon the instrument are almost exactly the same, as when it is in ordinary use. The Astronomer Royal has borrowed this principle (although he has applied it in a different form), in the apparatus which he has adopted for raising the proposed transit-circle for the Royal Observatory, in order to give opportunity for the adjustment of its collimators.

The general form of the axis, being unencumbered by any telescope crossing it, is evidently well adapted to the application of the level at all times; a thing which is always important, but particularly necessary for the German instruments, which are frequently counterpoised almost to the last ounce, so that there is not in them the same security for the bearing of the pivots in their proper position as in our instruments, in which a far greater residual weight is left.

The instrument, therefore, and its auxiliary apparatus, are most admirably adapted to securing the two advantages, of easy reversion and application of the level in the position of observation, which are so desirable for this instrument. But these advantages, in the opinion of the Astronomer Royal, are very dearly bought by an entire abandonment of that mechanical firmness of connexion

between the telescope and the axis which is obviously necessary to make the observations trustworthy. The kind of firmness which is required is that which retains the telescope in a position at right angles to its axis; the same, in fact, as that required for a transit-instrument. We laugh at the transit-instruments of the last century, in which, while great pains were taken to secure length of axis between the bearings, the central connexion was left very weak; and we praise the modern transit-instruments, in which the central connexion has been made successively larger and larger, and not least so by the German artists; and of which a more admirable specimen cannot be cited than the Edinburgh transit-instrument, made by Messrs. Repsold, the constructors of Struve's prime-vertical instrument. Yet in this prime-vertical instrument, that important connexion is probably very far weaker than in any transit-instrument that ever was made. The whole support of the 7-feet telescope, upon the firmness of which support the value of the observations entirely depends, is a single perforated pivot, 4 inches in diameter, on one side of the telescope. It is true that the telescope is sustained, as regards the strain of its own weight upon the small pivot, by an internal concealed counterpoise (for no one who has mastered all the external counterpoises of a German instrument is therefore to suppose that he has possessed himself of all the applications of that principle in the interior of the instrument), whose fulcrum is in the perforated pivot, near the telescope, and whose weight is within the hollow case at the other end of the axis, which appears to the eye like a large counterpoise connected with the external axis. But this counterpoise, while it delivers the pivot from the ordinary strains to which it would be exposed from the weight of the telescope, does in no degree diminish the effect of what, perhaps, are really more formidable, the accidental strains produced by pressures on the ends of the telescope, or other accidental forces, or irregularities of forces, not taken into account in the construction of the instrument. For instance, if the Y's were slightly irregular, so that the principal bearing of the pivots on the northern Y was an inch nearer to the face of the pier than that on the southern Y (a thing which it would be nearly impossible to discover by examination), the difference in the bend of the axis in the two positions of the instrument would probably be so considerable that every result would be worthless.

In the opinion of the Astronomer Royal, the asserted consistency of results hitherto obtained with this instrument proves nothing. Although discordance proves the existence of some fault, accordance does not negative the existence of very great faults. The Astronomer Royal cited the expression of Bouguer, who, after much painful experience in the construction of zenith sectors, in different forms, for the measure of the Peruvian arc, came at last to the conclusion, that no agreement of results proved their truth, unless the logical correctness of construction of the instrument gave reason *à priori* for believing that the results would be good.

One defect to which this instrument is liable was pointed out

by Mr. Struve himself to the Astronomer Royal. It cannot be assumed that the temperatures of the external faces of the two piers are the same, and if they be not, the effects of their radiation upon the telescope-tube must be different.*

The Astronomer Royal then remarked, that though perhaps the form of the ordinary transit-instrument would not give in their full extent the same facilities, yet the great importance of securing the admirable firmness and excellent connexions of the transit-instrument made it desirable for us to attempt to unite with them, as far as possible, the peculiar conveniences of Struve's instrument. In regard to the reversion, by a forked apparatus rising from the floor, there is no difficulty; the only difficulty is in the application of the level while the instrument is in the position of observation. There appears to be no valid reason prohibiting the use of one of the following constructions. The level-frame might consist of two bars extending from the pivot-forks towards the telescope, there interrupted in their straight course, and united by a large oval ring, through which the telescope could play; each bar must then carry a short spirit-level. Or it might consist of a parallelogrammic frame, strengthened in the middle by a ring through which the telescope could play, the two short sides being attached to the pivot-forks, and the two long sides carrying two long levels. But a different form of instrument may be suggested, allowing of the application of the ordinary single level at all times, and apparently embodying the conveniences of Struve's form, while it does not abandon the strength of the usual form. The transit-instrument may be made in the form of the letter T (the horizontal line of the T representing the axis of rotation), the object-glass of the telescope scarcely rising above the thick part of the axis. As the weight of the telescope is entirely on one side of the axis, it must be balanced by counterpoises carried by a very large fork; the stalk of the fork being within the telescope-tube, the two arms of the fork being in the axis of the transit, and resting within the pivots for a fulcrum, but projecting out beyond the pivots; and the two prongs of the fork projecting towards the object viewed by the telescope, and being loaded with the counterpoises at their ends.

If, however, we rely upon our transit taking the same bearing in the Y's after reversion, a very much simpler principle may be used, dispensing entirely with the level. It is only necessary, after having made the observations on the north side of the east prime vertical (as already described), to reverse the instrument and to observe on the south side of the east prime vertical by reflexion in a trough of quicksilver: then to reverse and observe on the south side of the west prime vertical by reflexion: then to reverse again, and to

* The Prime Vertical Instrument of M. Struve, constructed by Repsold, is fully figured in Plates xxxii. xxxiii. of M. Struve's magnificent work, *Description de l'Observatoire de Poulkova*, Saint-Pétersbourg, 1845. Drawings of the Greenwich Zenith Tube, and models of Struve's Prime Vertical Instrument and of the Reflex Zenith Tube, may be seen at the apartments of the Royal Astronomical Society.

observe, by direct vision, on the north side of the west prime vertical.

The Astronomer Royal expressed himself confident that in some of these ways the advantages of Struve's construction might be secured, with the additional guarantee for the goodness of the results, that they are obtained with an instrument of firm mechanical construction.

The Astronomer Royal then explained that his attention had been directed to these constructions by the necessity created by the present condition of astronomy for a few accurate observations, at whatever trouble obtained, of stars near the zenith. Struve's instrument is now employed on three stars only, and Mr. Struve is satisfied if of each of these stars he can obtain eight observations in a year. At Greenwich there is special need of observations of one star, namely γ *Draconis*, a star that may with propriety be considered as the birth-star of English astronomy. Unfortunately no instrument on the prime-vertical principle is applicable to this star, because it passes north of the zenith of Greenwich. The Astronomer Royal therefore has been compelled to endeavour to devise an instrument which shall be firm in its connexions, and shall also be applicable to the observations of stars on both sides of the zenith. The following is the construction which he proposes for this purpose.

III. The proposed Reflex Zenith Telescope is founded upon these considerations. If an object-glass be placed with its axis vertical; and if a pencil of light fall on it from a star near the zenith, and pass from the object-glass with its axis still inclined to the vertical, but with the rays of the pencil in a state of convergence; and if a trough of quicksilver be placed below it at a distance somewhat less than half the focal length of the object-glass; the pencil of light will then be reflected from the quicksilver with its axis still inclined in the same degree to the vertical, and with the rays still in the same state of convergence, and will again pass through the object-glass, and will form an image of the star at a very short distance above the object-glass, and at a distance from the axis of the object-glass depending on nothing but the star's zenith distance and the focal length of the object-glass. Although we cannot fix on the axis of the object-glass, yet we know that if the object-glass is turned through 180°, the image will now be formed at an equal distance from the axis of the object-glass, but in an opposite direction relatively to the frame of the object-glass; and therefore the distance between the two positions of the image, as measured by a micrometer attached to the frame of the object-glass, will be double the distance of either image from the axis of the object-glass, and will therefore be a measure of the star's zenith distance. The peculiar advantage of this construction is, that it requires no firmness of connexion except that of the micrometer with the frame of the object-glass. The mercury-trough may be totally unconnected with the rest of the instrument. The firmness of support of the object-glass is unimportant, for, however much the object-glass is pushed sideways (giving the same movement to the image of the

star), the micrometer is equally pushed sideways, and the measure of the image is not disturbed. The peculiar disadvantage is, that the light must be reflected from quicksilver, and must pass again through the object-glass, and must be transmitted through a four-glass eye-piece with a diagonal reflector, so that the whole loss of light will be considerable. But this disadvantage appears insignificant in comparison with the advantage.

Theoretically, the place of the image will be affected by a local fault in that part of the object-glass through which the rays pass the second time. But, practically, the existence of such a fault is unlikely: and its effect, if any existed, would be proportional to the distance of the image from the object-glass, and therefore small: and moreover it could be ascertained and measured by previous experiment on the object-glass. No injury therefore to the results, and no real inconvenience to the observations, would arise from such a fault.

The only risk to which this construction appears to be exposed is the following. If the object-glass with the micrometer attached be tilted, the place of the image of the star upon the micrometer will be disturbed by a small quantity, unless the plane of the micrometer be at one certain distance from the object-glass. It is therefore an important matter to determine what that certain distance will be. It is easily seen that if the plane of the micrometer pass through one of the points called *focal centres*, this condition is satisfied. For a ray from a vertical star passing through the focal centre (the object-glass being inclined), will be refracted in a parallel vertical direction to the quicksilver, and will then be reflected back from the quicksilver in the same line, and will by refraction be made to pass again through the same focal centre; and, supposing the distance of the quicksilver to be properly adjusted, so that the image of the star is formed on the micrometer, that image will be at the focal centre whatever be the inclination of the object-glass. The place of the focal centre may be determined by an apparatus, in which the object-glass is planted in a frame that admits of being slid in a direction perpendicular to its plane, the sliding-cell being upon a board which turns in its own plane on a pin; a beam of light is directed upon the lens through two narrow slits; and a telescope is placed on the opposite side of the object-glass to receive the light; when, by trial of sliding the frame, a position is so determined that, upon rotating the turning-board through a large angle, the position of the beam of light as seen in the telescope does not change, it is then certain that the focal centre is in the axis of rotation. If the micrometer can be conveniently fixed at this distance from the object-glass, the accidental inclination of the object-glass will be unimportant; if the micrometer is at any other distance, there will be a very small correction to the measures, depending on the inclination of the object-glass; and it will be proper that a small spirit-level be attached to the object-glass frame for the measure of the inclination. Perhaps in any case this addition will be prudent.

The adjustment to focal length will depend upon nothing but the distance between the object-glass and the quicksilver; and a power of altering this distance must be retained. It is proposed to do this by moving the tube, in which the object-glass turns, up or down by a rack-and-pinion motion, the tube and its load being as nearly as possible balanced by a lever-counterpoise. It is also proposed that a smaller quicksilver-trough, communicating with the larger, should carry a float, from which a light stalk should rise by the side of the object-glass frame; if this stalk be made of the same material as the micrometer, a scale upon the stalk will indicate the value of the micrometer-scale as corrected for thermal expansion, and as affected by any change of focal length.

Although such an instrument may be adapted to the observation of any stars which pass within a field of view expressed by the breadth of the object-glass, yet some conveniences of fixation are gained by limiting it to the one star γ *Draconis*. The following are the details of mounting proposed by the Astronomer Royal. The micrometer necessarily requires two metallic bars crossing the object-glass; and upon reversing the object-glass, with micrometer attached, the two bars will again occupy the same position in space. Consequently there will be no additional interruption of light if the support of the prism-reflector of the eye-piece be two bars in the vertical planes which pass through the micrometer bars, carried by crooked projections from the tube in which the object-glass turns (and therefore not reversed with the object-glass). And as the prism-reflector intercepts a small portion of the object-glass excentrically, a corresponding portion equally excentric on the opposite side must be intercepted by a small plate carried by the two bars, in order that the diffraction-disturbance of the star's image may be symmetrical. One lens of the eye-piece will be below the prism-reflector, and one close to its vertical, or nearly vertical, face (unless it be thought preferable to produce the effect of lenses, by grinding the faces of the prism-reflector to spherical forms): the remaining lenses of the eye-piece (namely, the field-glass and the eye-glass) will be fixed in a tube, entirely exterior to the object-glass and therefore causing no additional loss of light, carried by a crooked projection from the tube in which the object-glass turns. These crooked projections permit the micrometer-heads, and reversing-handle, &c. to pass, in the reversion of the object-glass.

Notwithstanding the great simplicity and compactness of the essential parts of this instrument, the Astronomer Royal thinks it desirable that it be so arranged that a double observation can be made at each transit of the star. It is necessary for this purpose that two wires be fixed in the micrometer plate, at an interval corresponding nearly to the double zenith distance of the star. By fixing temporarily in the immediate field of view of the eye-piece a cross of wires, or by planting a microscope for the occasion above the micrometer frame, the interval between these two wires may be found very accurately in terms of the revolutions of the micrometer; and by fixing other wires on the micrometer plate at intervals as

nearly as possible equal to that interval, and by using a series of microscopes fixed for the occasion, the micrometer-scale-intervals between all these wires may be very accurately found. And, by turning the object-glass and attached micrometer to a position 90° distant from either of the positions of observation, and observing the transits of zenithal stars over all the wires, the intervals in arc may be found. The combination of these will give the best possible information on the value of the micrometer-scale, and on the intervals of the wires.

A simple micrometer might be used for the observation; it would, however, have these disadvantages, that the micrometer must be read between the two observations, and that the observer could not use the same hand in the two actions upon the micrometer head. The Astronomer Royal proposes a more complex micrometer, in which the micrometer B, to which the bisection-wires are attached, is carried by and has its screw-*appui* in a micrometer A; and the micrometer A has its screw-*appui* in the cell of the object-glass. There is no difficulty in so arranging this that the movement of micrometer A shall not tend to disturb the relative place of micrometer B, and that the movement of micrometer B shall not tend to disturb the absolute place of micrometer A, (A and B standing in exactly the same relation as the tangent-screw of a mural circle, and the micrometer in its telescope): and then the observation may be conducted in this manner. One wire being very nearly in the position for bisecting the star, A will be read, and the bisection will be completed by B. Without waiting to read the micrometer head, the object-glass, &c. will be reversed, and the second bisection will be completed by A. Then A and B will be read. It will be proper sometimes to effect the reversion in the opposite order: and for this purpose, using the same hand on the micrometer, B must be read before beginning, and the first bisection must be completed by A, and after reversion the second bisection must be completed by B. In either case the complete double observation may be obtained with great rapidity, but without the smallest hurry.

The Astronomer Royal expressed his belief that an instrument thus constructed might be expected usually to give results accurate to one-tenth of a second of arc.

The Astronomer Royal then stated that, before attaching any name to this construction, he had requested the assistance of the Master of Trinity College, Cambridge, whose authority in a philological question of this kind is undisputed. Doctor Whewell has fixed on the name "THE REFLEX ZENITH TELESCOPE," a name which appears to express with singular accuracy the peculiarities of its construction, and which the Astronomer Royal hoped would be universally adopted.

METIS.

Ephemeris at Greenwich Mean Midnight. By Mr. Graham.

1849.	R.A. h m s	N.P.D. ° ′ ″	1849.	R.A. h m s	N.P.D. ° ′ ″
July 12	22 44 38·70	106 29 44·8	Aug. 21	22 21 8·20	110 27 37·3
13	44 33·65	33 53·7	22	20 10·11	33 35·5
14	44 26·91	38 11·7	23	19 11·73	39 26·8
15	44 18·46	42 38·7	24	18 13·13	45 10·7
16	44 8·30	47 14·5	25	17 14·38	50 46·8
17	43 56·44	51 59·1	26	16 15·56	110 56 14·6
18	43 42·85	106 56 52·1	27	15 16·77	111 1 33·8
19	43 27·56	107 1 53·3	28	14 18·06	6 43·9
20	43 10·55	7 2·6	29	13 19·53	11 44·5
21	42 51·84	12 19·7	30	12 21·24	16 35·4
22	42 31·44	17 44·3	31	11 23·28	21 16·2
23	42 9·34	23 16·2	Sept. 1	10 25·71	25 46·5
24	41 45·57	28 55·1	2	9 28·61	30 6·2
25	41 20·13	34 40·5	3	8 32·05	34 14·8
26	40 53·05	40 32·3	4	7 36·12	38 12·3
27	40 24·33	46 30·0	5	6 40·86	41 58·2
28	39 54·00	52 33·6	6	5 46·36	45 32·5
29	39 22·09	107 58 42·3	7	4 52·70	48 54·9
30	38 48·61	108 4 55·9	8	3 59·93	52 5·2
31	38 13·59	11 14·0	9	3 8·13	55 3·3
Aug. 1	37 37·06	17 36·2	10	2 17·36	111 57 49·0
2	36 59·04	24 2·2	11	1 27·68	112 0 22·1
3	36 19·56	30 31·5	12	22 0 39·18	2 42·6
4	35 38·65	37 3·6	13	21 59 51·91	4 50·4
5	34 56·34	43 38·2	14	59 5·93	6 45·3
6	34 12·68	50 14·8	15	58 21·28	8 27·4
7	33 27·70	108 56 52·9	16	57 38·03	9 56·6
8	32 41·42	109 3 32·0	17	56 56·24	11 12·8
9	31 53·91	10 11·7	18	56 15·96	12 16·1
10	31 5·22	16 51·3	19	55 37·22	13 6·6
11	30 15·40	23 30·5	20	55 0·07	13 44·2
12	29 24·51	30 8·7	21	54 24·55	14 9·0
13	28 32·59	36 45·3	22	53 50·70	14 21·1
14	27 39·70	43 19·9	23	53 18·55	14 20·6
15	26 45·92	49 51·8	24	52 48·13	14 7·6
16	25 51·29	109 56 20·6	25	52 19·47	13 42·1
17	24 55·90	110 2 45·7	26	51 52·58	13 4·3
18	23 59·83	9 6·4	27	51 27·48	12 14·5
19	23 3·14	15 22·2	28	51 4·18	11 12·7
Aug. 20	22 22 5·91	110 21 32·6	Sept. 29	21 50 42·71	112 9 59·1

1849.	R.A. h m s	N.P.D. ° ′ ″	1849.	R.A. h m s	N.P.D. ° ′ ″
Sept. 30	21 50 23·08	112 8 33·7	Oct. 16	21 49 21·62	111 21 23·2
Oct. 1	50 5·29	6 56·8	17	49 33·46	17 2·1
2	49 49·35	5 8·6	18	49 47·09	12 32·0
3	49 35·27	3 9·2	19	50 2·50	7 53·0
4	49 23·05	112 0 58·8	20	50 19·67	111 3 5·2
5	49 12·70	111 58 37·3	21	50 38·59	110 58 8·9
6	49 4·22	56 5·0	22	50 59·22	53 4·1
7	48 57·60	53 22·2	23	51 21·56	47 51·0
8	48 52·85	50 29·0	24	51 45·57	42 29·8
9	48 49·96	47 25·4	25	52 11·25	37 0·5
10	48 48·93	44 11·6	26	52 38·56	31 23·3
11	48 49·77	40 47·8	27	53 7·46	25 38·4
12	48 52·46	37 14·1	28	53 37·95	19 45·9
13	48 57·00	33 30·7	29	54 10·00	13 45·8
14	49 3·38	29 37·6	30	54 43·58	7 38·4
Oct. 15	21 49 11·59	111 25 35·0	Oct. 31	21 55 18·67	110 1 23·7

Horizontal Parallax.

	″		″		″
July 12	5·2	Aug. 21	6·1	Sept. 30	5·5
16	5·3	25	6·1	Oct. 4	5·4
20	5·4	29	6·1	8	5·3
24	5·6	Sept. 2	6·1	12	5·2
28	5·7	6	6·0	16	5·1
Aug. 1	5·8	10	6·0	20	5·0
5	5·9	14	5·9	24	4·8
9	5·9	18	5·8	28	4·7
13	6·0	22	5·7	Oct. 31	4·7
Aug. 17	6·1	Sept. 26	5·6		

"From the observations made in 1848, and one by Professor Challis, on June 22, 1849,—allowing for the perturbations caused by the *Earth*, *Mars*, *Jupiter*, and *Saturn*,—I have obtained the following set of elements for the instantaneous ellipse at the epoch.

1849, Aug. 23·0, Greenwich M.T.

	° ′ ″	
M	272 38 13·0	
$\pi - \Omega$	2 33 35·7	Mean Equinox,
Ω	68 28 58·2	1849, Aug. 23.
i	5 35 35·3	
φ	7 2 13·6	
Log a	0·3778018	
μ''	962″·2852	

"In calculating the ephemeris, the ellipse was assumed to be invariable. The calculated places have been corrected for aberration, and are referred to the apparent equinox."

FLORA.

HAMBURG. (M. Rümker.)

1849.	Hamburg M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″	
Feb. 17	15 19 30·2	220 17 0·3	−7 58 27·2	Equatoreal.
24	16 53 32·1	220 55 33·1	49 44·9	—
27	15 27 40·0	221 5 47·3	43 55·9	—
March 3	16 13 42·6	13 57·9	34 17·2	—
5	17 7 13·5	221 15 37·1	7 28 48·0	—
19	12 38 38·7	220 44 34·1	6 35 7·4	—
31	10 59 0·0	219 2 39·2	5 38 11·7	—
May 3	11 18 39·5	211 22 15·4	2 50 33·0	Mer. Circle.
4	13 45·9	211 7 46·9	46 58·9	—
5	11 8 52·8	210 53 26·4	43 31·2	—
12	10 35 4·1	209 18 51·5	23 28·2	—
18	10 6 45·6	208 7 56·0	12 52·3	—
20	9 57 29·7	207 46 54·0	10 58·0	—
21	52 53·8	36 48·7	10 11·8	—
22	48 19·3	27 5·9	9 46·4	—
24	39 14·5	207 8 50·4	8 59·9	—
26	9 30 13·3	206 51 30·0	9 23·8	—
30	10 52 37·6	206 22 36·7	11 45·5	Equatoreal.
June 11	11 43 13·6	205 33 37·3	35 45·9	—
13	11 31 16·1	31 3·7	41 38·2	—
14	11 28 26·5	30 22·5	45 10·1	—
15	11 12 50·0	205 30 6·7	−2 48 34·0	—

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1849.	Greenwich M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Comp^{n}—Obs^{n}. s ″		Stars B.A.C.	
May 28	12 26 50·1	13 46 22·64	92 10 4·4	+1·36	+10·7	4604,	4665
June 5	12 1 47·4	43 22·40	20 44·7	1·64	9·0	4547,	4665
6	11 41 10·6	43 7·13	22 51·1	1·73	6·4	—	—
7	11 45 14·6	42 53·43	25 6·3	1·57	6·9	—	—
9	11 52 11·4	42 30·42	30 4·5	1·69	8·6	—	—
10	11 40 8·1	42 21·66	32 47·9	1·46	7·6	—	—
13	11 28 1·3	42 3·85	41 51·2	1·60	8·6	—	—
17	11 44 44·3	42 2·18	56 3·3	1·48	10·2	—	—
18	10 50 56·9	13 42 5·38	92 59 52·1	+1·54	+ 6·4	—	—

"The observed places are corrected for refraction and parallax. The computed places were deduced from Dr. Brünnow's Ephemeris, published in the *Monthly Notice*, vol. ix. No. 7. The places of the stars are taken from the catalogue cited. The right ascension of 4604 appears, by the other stars with which *Flora* is compared, to be about $0^{s}\cdot5$ too great. The correction to the Ephemeris in right ascension from 4665 alone on May 28 $= +1^{s}\cdot62$.

"The results on June 6th and 18th are each deduced from one observed transit over five wires and the readings of two microscopes; on all the other days the observation was repeated, and the mean of the two taken."

IRIS.

HAMBURG. Meridian Circle. (M. Rümker.)

1849.	Hamburg M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″
Feb. 11	12 32 13·3	149 58 31·9	+3 9 17·4
17	12 2 30·1	148 26 20·0	3 37 29·3
23	11 32 57·6	146 56 49·3	4 8 24·2
24	28 4·6	42 28·3	13 37·8
27	11 13 30·2	146 0 42·7	4 29 40·0
March 6	10 40 8·6	144 32 54·5	5 6 44·4
8	30 48·2	10 42·7	17 1·0
9	26 10·2	144 0 13·6	22 8·4
12	12 24·7	143 30 42·4	36 55·7
13	7 53·0	21 44·2	41 43·6
14	10 3 22·8	143 13 8·0	5 46 30·6
19	9 41 15·6	142 36 3·4	6 8 55·3
30	8 55 4·7	141 51 56·6	49 2·8
31	51 3·2	50 29·8	6 52 3·0
April 5	31 19·7	49 33·4	7 4 58·9
6	8 27 28·2	141 50 37·4	+7 7 9·6

HEBE.

HAMBURG. (M. Rümker.)

1849.	Hamburg M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″	
Feb. 11	8 9 36·4	84 8 29·4	+11 54 56·7	Mer. Circle.
17	7 48 4·6	39 29·7		—
18	44 34·7	84 46 2·0	13 1 9·1	—
21	34 16·3	85 8 24·5	28 22·6	—
23	7 27 32·1	85 25 21·7	13 46 5·1	—
March 6	6 52 13·1	87 24 40·5	15 16 47·8	—
14	8 44 12·9	89 17 37·6	16 15 43·2	Equatoreal.
19	9 51 2·7	90 37 44·3	+16 48 59·0	—

NEPTUNE.

Ephemeris for Greenwich Mean Noon (Encke's *Jahrbuch* 1851).

	R.A. h m s	N.P.D. ° ′ ″		R.A. h m s	N.P.D. ° ′ ″
July 9	22 26 45·30	100 30 36·8	Oct. 7	22 18 35·79	101 18 30·0
19	26 4·24	34 51·7	17	17 57·98	21 58·8
29	25 15·01	39 51·7	27	17 30·87	24 25·0
Aug. 8	24 19·42	45 27·4	Nov. 6	17 15·65	25 42·6
18	23 19·46	51 21·1	16	17 13·10	25 47·7
28	22 17·45	100 57 24·1	26	17 23·69	24 38·3
Sept. 7	21 15·74	101 3 21·7	Dec. 6	17 47·22	22 15·5
17	20 16·68	9 0·6	16	18 23·45	18 42·7
27	22 19 22·64	101 14 7·2	26	19 11·38	14 2·7

Adams' Elements: Hor. Par. = 0″·3.

HYGEIA.

NAPLES.*

	Naples M.T. d	R.A. ° ′ ″	Dec. ° ′ ″
1849. April	14·3771	182 57 57	−7 28 18
	17·5854	182 28 11	7 13 10
	22·3840	181 49 20	6 52 6
	23·3563	41 38	47 31
	25·4096	27 19	39 21
	26·3598	20 56	35 37
	27·4071	14 38	31 48
	29·3745	181 3 14	24 26
May	1·3823	180 53 1	17 9
	5·4768	35 57	6 3 57
	7·3890	30 3	5 58 40
	8·2877	27 21	55 49
	13·4160	18 53	45 13
	15·3961	17 57	40 24
	16·3988	180 18 2	−5 39 3

PADUA.* (Professor Santini.)

	Padua M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″
May 27	10 38 41	180 39 50	−5 29 42
29	10 3 4	180 47 47	−5 29 12

"It is not *quite* certain that the planet was observed at Padua, but it appears probable that both R.A. and Declination of May 27 and R.A. of May 29 are those of the planet."

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1849.	Greenwich M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Star B.A.C.
June 5	10 30 44·6	12 5 45·10	95 34 51·2	4172
6	11 5 14·7	12 6 13·13	95 36 14·2	4080
7	10 35 37·5	12 6 40·12	95 37 27·1	—
9	10 40 45·3	12 7 39·07	95 40 32·8	—
10	10 45 8·5	12 8 10·09	95 42 16·9	—
13	10 36 31·8	12 9 49·27	95 48 9·2	—
17	10 50 42·1	12 12 16·45	95 57 40·0	—
18	10 24 53·2	12 12 54·98	96 0 13·4	—

"The observations are corrected for refraction and parallax. The horizontal parallax was taken from Mr. Graham's Ephemeris. The places of the stars of comparison are taken from the catalogue cited.

"The planet appeared to me a little, but not much, fainter than *Flora*. I was favoured by Professor Schumacher with an account of its discovery as early as the 14th May, and subsequently the Astronomer Royal supplied me with an ephemeris by Mr. Breen; but the weather was so unfavourable here that I did not succeed in finding the planet till the 5th June."

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* Communicated by Professors Schumacher and Santini to the President. M. de Gasparis referred the naming of his new planet to M. Capocci, who has called it *Hygeia*.

SOUTH VILLA. (MM. Bishop and Hind.)

	Greenwich M.T.	R.A.	N.P.D.
	h m s	° ′ ″	° ′ ″
May 27	9 51 45	180 39 40·5	95 29 50·4
June 1	10 32 53	181 2 35·9	31 25·1
2	9 58 19	181 7 52·2	32 6·1
10	9 41 10	182 1 49·7	42 2·8
13	9 53 1	182 26 43·4	95 47 54·6

HAMBURG. Equatoreal. (M. Rümker.)

1849.	Hamburg M.T.	R.A.	Dec.
	h m s	° ′ ″	° ′ ″
May 22	10 28 35·5	180 24 44·4	−5 32 4·4
24	10 38 10·4	29 48·3	30 48·6
26	10 36 53·3	36 0·9	30 9·8
28	10 30 49·8	180 43 39·7	−5 30 4·5

GOUJON'S COMET.

Ephemeris. By Mr. Pogson.

For Greenwich Mean Midnight.

	R.A.	Dec.		R.A.	Dec.
	h m s	° ′ ″		h m s	° ′ ″
May 28	11 24 49·7	+60 49 29	June 20	12 11 58·0	+69 13 54
29	26 20·9	61 22 53	21	14 40·5	27 8
30	27 54·7	61 54 46	22	17 26·8	39 51
31	29 31·1	62 25 16	23	20 16·9	69 52 5
June 1	31 10·2	62 54 28	24	23 10·9	70 3 50
2	32 52·1	63 22 27	25	26 8·8	15 7
3	34 36·7	63 49 16	26	29 10·7	25 57
4	36 24·1	64 15 0	27	32 16·6	36 19
5	38 14·3	64 39 42	28	35 26·5	46 15
6	40 7·1	65 3 25	29	38 40·6	70 55 45
7	42 2·8	26 11	30	41 59·0	71 4 49
8	44 1·8	65 48 4	July 1	45 21·5	13 28
9	46 4·0	66 9 5	2	48 48·2	21 42
10	48 8·9	29 18	3	52 19·0	29 30
11	50 16·8	66 48 45	4	55 54·1	36 54
12	52 27·9	67 7 28	5	12 59 33·3	43 52
13	54 42·2	25 28	6	13 3 16·7	50 26
14	56 59·8	42 48	7	7 4·2	71 56 34
15	11 59 20·7	67 59 28	8	10 55·9	72 2 18
16	12 1 45·1	68 15 31	9	14 51·7	7 36
17	4 12·9	30 58	10	18 51·8	12 30
18	6 44·3	68 45 50	11	22 56·0	16 59
June 19	9 19·3	+69 0 8	July 12	13 27 4·4	+72 21 4

	497″·8 × Δ	Hor. Par.		497″·8 × Δ	Hor. Par.		497″·8 × Δ	Hor. Par.
	m s	″		m s	″		m s	″
May 28	6 40	10·68	June 13	8 39	8·24	June 29	10 16	6·94
June 1	7 11	9·90	17	9 5	7·84	July 3	10 36	6·71
5	7 42	9·25	21	9 30	7·49	7	10 55	6·52
9	8 11	8·71	25	9 54	7·19	11	11 13	6·35

"The foregoing places are reckoned from the true equinox of the day. The elements employed are those of M. D'Arrest, as given in the last *Monthly Notice* of the Society," page 161.

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1849.	Greenwich M.T.	R.A.	Log $\frac{p}{P}$	N.P.D.	Log $\frac{q}{P}$	Star B.A.C.
	h m s	h m s		° ′ ″		
May 23	12 17 57·8	11 18 3·30	+8·853	32 13 49·5	−9·447	3959
28	13 56 38·5	11 25 7·21	8·905	28 58 57·8	9·689	—
June 5	13 19 16·0	11 38 35·60	8·970	25 9 24·3	9·587	4036
9	13 15 25·0	11 46 28·69	8·992	23 39 53·7	9·590	4074
13	12 25 42·5	11 55 8·00	9·018	22 23 45·3	9·537	4122
19	11 58 56·5	12 9 46·73	9·044	20 49 34·3	9·313	4222
23	12 4 34·2	12 20 50·32	+9·065	19 57 28·0	−9·347	—

"The observations are corrected for refraction. The corrections to be applied for parallax in time and arc are represented by p and q. P is the equatoreal horizontal parallax.

"The observations were all made with illuminated wires in a dark field. The comet was bright enough to admit of the wires being illuminated in this way, even when the moon was full and near the meridian.

"The places of the stars are taken from the catalogue cited. The positions of all, except 4222, depend wholly on Groombridge.

"On the 23d June, the comet had the appearance of a faint nebulous star. The nucleus was decidedly stellar, and about equal to a star of the twelfth or thirteenth magnitude. Power 134 was used for all the observations."

STARFIELD. 20-foot Equatoreal. (Mr. Lassell.)

1849.	Starfield Sid. Time.	R.A. Star—Comet.	Starfield Sid. Time.	N.P.D. Star—Comet.
	h m s	m s	h m s	″
June 23	18 48 6·1	2 31·07	18 47 36·2	27·20
	19 5 6·9	2 28·40	19 4 36·9	38·03
	19 19 22·6	2 26·67	19 18 52·9	43·95

Star of comparison, B.A.C. 4222.

"Magnifying power 219, applied to a micrometer of my own construction. The comet had a very minute stellar disc, and was easily observed. No illumination used, the twilight rendering the bars of the micrometer sufficiently visible."

SOUTH VILLA. (MM. Bishop and Hind.)

	Greenwich M.T.	R.A.	N.P.D.
	h m s	° ′ ″	° ′ ″
May 23	10 0 58	169 29 28·4	32 18 9·5
June 11	10 15 30	177 36 57·3	23 2 29·8

HAMBURG. Equatoreal. (MM. C. & G. Rümker.)

1849.	Hamburg M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″	
April 20	8 52 9·8	166 8 36·7	− 9 49 0·5	C. R.
26	11 48 59·1	170 30 43·1	59 45 28·9	G. R.
28	11 38 36·5	171 13 42·9	60 56 49·1	—
June 11	12 21 5·4	177 38 35·4	66 58 39·4	—
13	12 38 44·9	178 46 27·7	67 35 32·8	—
14	12 35 57·5	179 21 0·9	67 53 8·5	—
15	12 27 21·3	179 56 3·2	68 9 30·0	—
17	11 35 2·4	181 8 46·2	+68 40 56·0	—

HAVERHILL. (W. W. Boreham.)

1849.	Greenwich M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Star of Comp.
May 3	11 7 47	11 2 16·95	59 23 9·9	H.C. 21331
7	10 28 53	3 45·10	51 7 59·5	— 21364
13	9 47 41	7 34·11	41 58 58·7	Arg. Z. 178 : 73
15	11 14 59	9 20·66	39 31 51	B.A.C. 3846
22	11 2 16	16 44·68	33 1 45	Arg. Z. 100 : 22
23	11 0 42	17 59·54	32 16 49·4	Arg. Z. 199 : 19
27	10 46 44	11 23 17·07	29 38 0·2	Arg. Z. 184 : 37

"The first two observations are corrected for refraction and parallax; in the others these corrections are neglected.

"The absence of any stellar nucleus in this comet renders it difficult to observe."

SCHWEITZER'S COMET.

HAMBURG. Equatoreal. (M. Rümker.)

1849.	Hamburg M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″
May 3	9 46 9·2	145 7 42·5	−11 47 18·6

On the Orbits of Double Stars. By M. Yvon Villarceau.

(*Third Notice.*)

ξ Ursæ Majoris.

"M. Savory, who first attempted the problem of determining the elliptic motion of double stars, applied, as is known, his formulæ to the binary system *ξ Ursæ Majoris.*

"M. Savory has only given his result as an example of the computation, pointing out the probability of a great difference between his elements and the true elements. The observations at his disposal only extended to 1827, and it is remarkable that his determinations do not differ more widely from those which are deduced

from the observations of 20 additional years. M. Savory's greatest difficulties must have occurred in passing from the observed positions to those which he has substituted, which are the base of his calculations. It is to be regretted that the learned Academician has not thought it necessary to enter into the details.

"Mr. Mädler has since occupied himself with the same star, using observations which extend to 1841. The elements which he has deduced do not differ considerably from those which I have the honour to present.

"The system of ξ *Ursæ Majoris* consists of two stars. These are, according to M. Struve, of the 4th and 5th mag. respectively. M. M. Herschel and South estimate them of the 6th and $6\frac{1}{4}$ mag.

"The earliest observation of ξ *Ursæ Majoris* was made by Sir W. Herschel in 1781·97; he reobserved it again in 1802 and 1804. The last two observations are only equivalent to one distinct position in regard to the determination of elements. From 1819 to the present time numerous observations have been made by different astronomers, and particularly by M. M. Struve and Sir John Herschel. The series is almost continuous.

"I have obtained a first approximation to the elements by the new method which I presented to the Academy at its sitting on March 26. For this purpose and to simplify the computations, I united in six groups, as well for the angles of position as for the distances, 20 observations of position and 17 of distance, extending from 1819 to 1847. A preliminary graphical construction had shewn me the possibility of grouping the observations without notable error. I have found, as I had previously expected, that these observations alone, though embracing a period of 28 years and containing an angular displacement of 152°, were not by themselves sufficient even for an approximate value of the elements. For example, one could satisfy them within the limit of moderate errors with a periodic time of 166 years, and even longer, or with one of about 61·5 years. Besides this, the problem then becomes doubly indeterminate, as two of the constants on which the elements depend may receive independent variations. The ancient observations are necessary, and sufficient to destroy the double indetermination: comparing that of 1781·97 with the modern observations, we obtain at once 62 years as an approximate value of the periodic time. From this the mean motion is deduced, whence results an equation of condition between the two indeterminate constants, from which one of these may readily be expressed in a function of the other. In this manner only one arbitrary quantity is left, the value of which is fixed by the condition of satisfying a single angle of position, the result of the two observations of 1802 and 1804. The degree of approximation at which I have thus arrived is tolerably satisfactory, notwithstanding the employment of somewhat indifferent observations from 1819 to 1823, the effect of which is to alter the calculated angles of position for that interval.*

* Many of these observations were made with meridian instruments.

"If my only object had been that of obtaining an orbit of ξ *Ursæ Majoris*, this first determination would have been sufficient. But it is important for the discussion of observations, and the perfecting of micrometrical measurements to which this discussion may lead, to obtain the highest possible degree of precision. I have therefore set myself to correct my first elements, by employing all the known angles of position (two alone excepted) and all the measures of distance made by M. M. Struve since 1825. I have also employed one observation of 1828, which M. Otto Struve has had the kindness to send me, and which I did not receive till after the first portion of my work was completed. By means of the errors resulting from a comparison of observations with the approximate elements, I have formed eight groups for the angles of position and five for the distances. From these I have deduced *normal* positions, which I have substituted for the observations. The elements corrected by using these *normal* positions, and compared with the observations, have given for each of the above groups errors, the means of which coincide very nearly with the errors resulting from a comparison with the *normal* places. This proves that I should not sensibly have obtained greater precision by employing all the observations than by employing the *normal* positions.

"I present here the approximate and corrected elements, and also a comparison of each with the observations: —

Elements of the relative Orbit of ξ *Ursæ Majoris*.

1826·0 R.A. = $11^h\ 8^m\cdot8$ N.P.D. 57° 30′, 4 and 5 mag.

	First Approximation.	Second Approximation.
True Perihelion Passage....	$1817^y\cdot436$ & $1879^y\cdot436$	$1816^y\cdot859$ & $1878^y\cdot435$
Mean Annual Motion.....	5°·8064	5°·8464
Angle (sin = excentricity)...	26 ° 2′·0	25 ° 33′·7
Long. Asc. Node, reckoned from the apparent North in 1834·4 towards the East	276 ° 5′·5	275 ° 50′·0
Dist. of Per. to Asc. Node..	311 ° 5′·3	308 ° 57′·2
Inclination	±128 ° 22′·6	±127 ° 11′·4
Semi-axis major (observations of MM. Struve)....	2″·4082	2″·4389

Hence I deduce,

	First Approximation.	Second Approximation.
Periodic Time	62·0 years.	61·576 years.
Excentricity.....	0·43891	0·43148

For the apparent orbit I find,

	Date.		Position.	Dist.
Greatest apparent Aphelion..	$1795^y\cdot111$ &	$1856^y\cdot687$	112 ° 46′·4	3″·0538
Least — —	1824·461	1886·037	247 ° 15′·2	1″·7187
Least apparent Perihelion...	1814·760	1876·336	356 ° 12′·5	0″·8891
Greatest — —	1828·881	1890·457	218 ° 25′·8	1″·6924

"The angles of position in this table are reckoned from the same origin as the ascending node.

Comparison of the Eleme

Date.	Observations. Angles of Position.	Distance.	Magnifying Power.	Number of Days.	Observer.
	° ′	″			
1781·97	143 47			1	W. Herschel.
1802·09	97 31			1	—
1804·08	92 38			1	—
1819·10	284 33			2	W. Struve.
1820·13	276 21			3	—
1821·78	264 42			3	—
1823·29	258 27			4	Herschel & Sout
1825·22	242 32				South.
1826·20	238 45	1·747	600	3	W. Struve.
1826·20	238 17				South.
1827·27	228 16	1·715	570	4	W. Struve.
1828·37	224 1				J. Herschel.
1829·02	219 0				—
1829·35	213 35	1·671	583	7	W. Struve.
1830·58	206 18				J. Herschel.
1831·08	201 32				Bessel.
1831·25	201 7				J. Herschel.
1831·34	201 55				Dawes.
1831·44	203 49	1·706	600	5	W. Struve.
1832·16	198 10				J. Herschel.
1832·27	196 43				Dawes.
1832·41	195 56	1·750	600	5	W. Struve.
1833·14	189 58				J. Herschel.
1833·23	189 50				Dawes.
1833·84	188 25	1·762	1000	5	W. Struve.
1835·41	180 11	1·764	920	5	—
1836·44	171 12	1·972	800	4	—
1837·47	165 19	1·927		3	Struve, *Additam.*
1837·53	167 23				Encke.
1838·43	160 23	2·260		9	Struve, *Additam.*
1839·47	157 58				Galle.
1840·25	152 14				Kaiser.
1840·40	155 24	2·286	858	7	W. & O. Struve.
1841·29	150 12				Mädler.
1841·402	152 8	2·282	858	6	O. Struve.
1842·40	148 47	2·410	858	4	—
1844·788	140 52	2·526	858	5	—
1846·365	138 49	2·620	858	4	—
1847·407	131 50	2·700	858	3	—
1848·406	128 43	2·752	858	5	—

with the Observations.

First Approximation.			Second Approximation.		
Angles of Position.		Distance.	Angles of Position.		Distance.
Calculated—Observed.		Cal^{d}—Obs^{d}.	Calculated—Observed.		Cal^{d}—Obs^{d}.
Diedral.	In Arc.		Diedral.	In Arc.	
° ′	″	″	° ′	″	″
−0 22	−0·016		+0 16	+0·011	
+0 42	+0·034		+0 11	+0·009	
0 32	0·024		−0 8	−0·006	
5 34	0·129		+1 0	+0·025	
3 54	0·099		0 26	0·012	
+2 21	+0·065		+0 4	+0·002	
−2 8	−0·061		−3 40	−0·110	
+0 40	+0·019		0 10	0·005	
−2 10	−0·063	−0·080	2 43	0·081	−0·038
−1 42	−0·049		−2 15	−0·067	
+1 4	+0·031	−0·051	+0 47	+0·023	−0·015
−2 8	−0·062		−2 13	−0·065	
−1 31	−0·044		−1 30	−0·044	
+1 40	+0·049	0·000	+1 44	+0·051	+0·022
0 45	0·022		0 55	0·027	
2 15	0·067		2 26	0·073	
1 34	0·046		1 45	0·052	
+0 11	+0·006		+0 22	+0·011	
−2 22	−0·071	+0·002	−2 10	−0·065	+0·013
1 18	0·039		1 6	0·033	
0 32	0·016		0 20	0·010	
−0 37	−0·019	−0·011	−0 25	−0·013	−0·006
+0 53	+0·027		+1 3	+0·032	
+0 28	+0·014		+0 39	+0·020	
−1 43	−0·054	+0·037	−1 34	−0·049	+0·036
−2 12	−0·073	+0·119	−2 10	−0·071	+0·112
+1 31	+0·051	−0·025	+1 26	+0·048	−0·034
2 28	0·087	+0·089	+2 18	+0·080	+0·078
0 7	0·004		−0 3	−0·002	
3 6	0·113	−0·176	+2 50	+0·102	−0·188
1 11	0·045		0 50	0·031	
3 51	0·149		+3 26	+0·132	
0 7	0·005	−0·055	−0 18	−0·012	−0·069
+2 4	+0·083		+1 34	+0·062	
−0 16	−0·011	+0·025	−0 47	−0·031	+0·012
−0 20	−0·014	−0·027	0 55	0·038	−0·040
+0 14	+0·010	+0·033	0 29	0·021	+0·022
−2 3	−0·096	0·047	−2 51	−0·132	0·038
+2 15	+0·107	0·034	+1 25	+0·067	0·025
+2 55	+0·142	+0·042	+2 2	+0·099	+0·034

"These two sets of elements do not present any remarkable difference; nevertheless, the correction of the first has sensibly diminished the differences between the calculated and observed places. In the second approximation the greatest errors in the distances belong to 1835 and 1838; the last is almost 0″·2. It is evident, on comparing the neighbouring observations, that the greatest share in these errors belongs to the observations themselves. The other distances are well represented. As to the angles of position, there are only four which vary from 0″·10 to 0″·13, and here the fault may be imputed chiefly to the observations. Taking a mean of the variations which exist with the corrected elements, I find 0″·046 both for angles of position and for distances. This result shews how advantageously the distances of M. M. Struve may be employed.*

"M. Struve estimates the probable error of a mean of three measures, when the distances are nearly those of the preceding table, to be 0″·05 for distances, and 0″·03 for angles of position. The mean error assigned above is rather less than M. Struve's for distances, and half as large again for angles of position. The agreement in the angles of position for this star is not quite so satisfactory as that which I found in my researches on *η Coronæ*. The differences in the second approximation shew permanences of sign which cannot be made to disappear by correcting the elliptic elements. As to the cause, it cannot be searched for here, in perturbing actions or in aberrations, since the observations are barely sufficient to determine the constants of elliptic motion."†

Note upon the Mass of the Planet Neptune, as deduced from Observations of his Satellite. By Mr. Hind.

"The satellite of *Neptune* has been frequently observed at Starfield by Mr. Lassell; at Cambridge, United States, by Professor Bond; and at Pulkova by M. Otto Struve. There are about forty complete observations of position and distance up to the end of October 1848.

"By supposing the inclination of the orbit of the satellite to the ecliptic = 30° and the place of the node in 300°, and by assuming

* I have not compared the distances observed by other astronomers, as they are not in sufficient numbers to give the value of the semi-axis major which corresponds to each observer.

† Calculated elliptic elements cannot be made to satisfy a series of distances, unless the distances are absolutely correct; a constant error, such as has been suspected from the varying results of different observers, will spoil the coincidence. It is much to be wished that observers who possess instruments of sufficient power would carefully and repeatedly measure the distances of *ξ Ursæ Majoris*, *η Coronæ*, &c., in order that something definite may be known of the extent and constancy of these personal or instrumental equations. Possibly the true measure might be approximated to, by assuming a correction for each observer, such as would make the elliptic elements, deduced from angles of position, agree best with his observed distances.

that a nodal passage occurred on 1847, Oct. $2^d{\cdot}745$, Greenwich Mean Time, and that the period (as indicated by the whole series of observations) is $5^d{\cdot}8750$, the angles of position observed by Mr. Lassell and Professor Bond are well represented; but the measures of M. Otto Struve present a marked disagreement in angle, the mean error of calculation, according to eleven observations by this astronomer, being $+6^\circ{\cdot}6$: the individual errors are uniformly in excess. I can discover no mistake in my computations, and can only suppose that we have here another instance of a constant personal equation affecting the measured angles of position, similar to that which has been pointed out by Sir John Herschel in Professor Struve's observations of *γ Virginis*, and in Professor Mädler's more recent measures of the same star.

"The distances present a similar anomaly. Treating the observations at the three observatories separately, I find,—

The Apparent Mean Distance of Satellite from Planet's centre,

At Distance = 30·0,	by Professor Bond's observations	$16''{\cdot}102$
——	— by Mr. Lassell's observations	16·423
——	— by M. Otto Struve's observations	18·060

"The values inferred from the English and American measures agree as nearly as can be expected from the number of observations employed. The Pulkova distance is nearly two seconds greater than this mean, a quantity sufficient to produce a very material alteration in the mass of the planet as determined by observations of the satellite.

"The cause of these differences may, perhaps, be explained hereafter; but the knowledge that such constant discordances may exist between different observers will probably tend to throw much uncertainty over the values of the planet's mass deduced from actual observation.

"If we assign to each of the above results its proper weight depending on the number of measures on which it is founded, we shall have for the radius of the satellite's orbit at the same unit of distance,

$$16''{\cdot}748,$$

and supposing the periodic time $5^d{\cdot}8750$, which must be close upon the truth, the mass of *Neptune* comes out

$$\frac{1}{17900\cdot}$$

"The distance of the satellite from the planet's centre appears to be 232·000 miles."

On the first Comet of Brorsen, 1846. By Mr. Hind.

"In the *Monthly Notice* of this Society for November 1846, I pointed out the remarkably close approach of the periodical comet

of Brorsen to the planet *Jupiter* in May 1842; the result there given being founded upon Dr. Brünnow's approximate elements. Having since calculated an orbit from the whole extent of observation, I recomputed the distances between the planet and comet between May 0 and June 9, and the results seem to establish the fact of a very near approach, though not quite so close as Dr. Brünnow's elements had indicated. My corrected orbit is as follows:—

Epoch of Mean Anomaly, 1846, March 0·0, G.M.T. = 0° 27′ 24″·6

Longitude of Perihelion..	116° 28′ 23″·9	Mean Equinox of Epoch.
Ascending Node.....	102 37 40·5	
Inclination	30 57 50·8	
Angle of Excentricity ...	52 36 15·2	
Log. Semi-axis Major	0·5000430	
Period of Revolution	5^{yrs}·624	

"Referring the co-ordinates of the comet and planet to the ecliptic, we find,

G.M.T. 1842.	Comet. x	y	z	Jupiter. x'	y'	z'
May 0	+1·1902	−5·0230	−0·0372	+1·0666	−5·0775	−0·0062
10	·2261	·0426	·0559	·1396	·0578	·0079
20	·2621	·0619	·0749	·2126	·0372	·0097
30	·2982	·0806	·0934	·2851	5·0155	·0114
June 9	+1·3344	−5·0987	−0·1120	+1·3576	−4·9926	−0·0131

whence the real distances between the comet and *Jupiter* will be,

May 0 = 0·1386
10 = 0·1001
20 = 0·0856
30 = 0·1055
June 9 = 0·1468

"The nearest approach would occur about May 19, on which day the distance would amount to 0·085, or 8,100,000 miles. These results are, of course, independent of the effect of perturbation between 1842 and 1846, but they appear to shew that the calculation of *Jupiter's* disturbances backward to the early part of the former year would conduce to some very interesting results. The similarity between the orbit of Brorsen's comet and those of the comets of 1532 and 1661, long supposed to be identical, may of itself be considered sufficient inducement to some one accustomed to these intricate computations to undertake the solution of the question with all possible accuracy."

Extracts of Letters from Mr. Lassell.

"You will, I am sure, be pleased to hear that I have gained a long march further in the art of figuring specula. I have successively refigured both the 20-foot mirrors, which are decidedly superior (especially A, that now in the tube) to the surface with which I made all my observations last year. I shall therefore enter the field with *Neptune*, *Uranus*, &c. on a vantage ground this year. The improvement I have made is not so much in the greater correctness of the general curve as in its greater regularity and continuity, producing superior definition, and neater, rounder, and harder disks of stars. I have attained a much more complete control over the polishing machine, and take to it now, without risk of injury and with strong probability of improvement, surfaces such as formerly I dared not touch. I am now fully persuaded of the competency of the machine to do all I ever hoped from it, and more than I ever hoped in giving correct curves with short foci. I have lately made a 12-inch mirror of $89\frac{1}{2}$ inches' focus without sensible error from the parabola, and sharper in definition than I had ever previously made a 9-inch mirror of 112 inches' focus. Stars are beautifully shewn by it, even the most difficult doubles, such as *ζ Herschel* and *ϑ Cygni*, the severest tests of telescopes. The best figures also on the 9-inch I have hitherto obtained are thrown into the shade by some recent surfaces, which shew me that a degree of excellence is attainable of which I had never before dreamt. Such pictures of stars I never saw before. These figures on the smaller specula, and the improved surfaces of the 20-foot, give me sanguine hopes of still further excellence in the latter, and to this point I am now bending my attention. I am surprised by the *quality* of vision of the short-focus telescope. I see no increase of loose light about a star, nor greater hardness of rings; nor should I know by any inferiority of vision that I was not looking with a focus of 12 diameters. I have improved the mode of supporting the 20-foot mirror."

Mr. Lassell mentions the following particulars respecting the fourth satellite of *Jupiter*. On the 13th of June, in the early part of the evening, this satellite seemed so small as to suggest the idea that it was a faint star which had got accidentally in the line of satellites. A little later, the satellite was again noticed with another telescope, and appeared *reduced to one-half its ordinary magnitude*. At about 10^h it was far from being obvious in an excellent Gregorian of 4·7 inches' aperture: indeed, it was not seen for some time by a young lady who is not unaccustomed to look through telescopes. Mr Lassell further remarks, "that the transits of the fourth satellite and its shadow over the disk of *Jupiter* seldom occur; in these cases, however, the satellite appears as a large dusky spot,

and the shadow is a black spot much larger than that of any other satellite, not excepting the third. This would seem to shew that the fourth is the largest satellite, though, when seen off the disc, it generally appears to be the smallest, and is always, I think, the least luminous."

"The variation of brightness seems to indicate a revolution on its axis. I have no doubt that in a less degree the third satellite is partially obscured by dark shades or spots; for when seen on the face of *Jupiter*, its form is an irregular oval, though the shadow is round."

Mr. Lassell proposes to re-examine these satellites with care when *Jupiter* is next in opposition.

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*On an improved Compensation-Balance.** By Mr. Hartnup, Director of the Liverpool Observatory.

"The Liverpool Observatory was established chiefly for rating and testing the chronometers of vessels which sail from that port. In a large majority of cases the seaman relies solely on his time-keepers for the determination of longitude, and is frequently unable to employ any other means. Hence it became my especial duty, as director of this establishment, to devote my attention particularly to everything which could improve the going of marine chronometers. On my appointment I gladly availed myself of the permission of the Astronomer Royal to make myself fully acquainted with the process of rating chronometers at the Royal Observatory, and I have mainly adopted the plan followed there.

"The chronometrical department of the Liverpool Observatory was opened to the public in the summer of 1844, and several chronometers belonging to North American packet ships were sent to be rated. The same chronometers were returned to the observatory in the following winter, after having made the voyage to New York and back. On comparing the land-rates given in the summer with the sea-rates made during the voyage, these did not differ very sensibly in those chronometers which had gone steadily at the observatory in the summer; but on rating them again in the chronometer-room, they were found to be *losing more* by from one to four seconds a-day. The chronometer-room had no fire in it, and the temperature, which in the summer had been about 60°, was now only 40°. As I supposed the change of rate arose from change of temperature, I removed the chronometers to my sitting-room, in which the temperature was about 60°, when they immediately returned to their former rates. The chronometers were removed

* The reader will bear in mind, that the fault of all watches furnished with the usual compensated balance is to *lose* in *extreme* temperatures when compensated for a *mean* temperature; or, what is the same thing, to *gain* in a *mean* temperature if going correctly in extreme temperatures.

from one room to the other several times, and the rates were changed, in consequence, with very great regularity and consistency. I became satisfied, from a few more experiments, that the chronometers ordinarily employed in the merchant-service were imperfectly compensated. I applied to my committee, and obtained leave to have an apparatus made for testing chronometers in heat, which, when finished, was perfectly satisfactory.

"As the transit-instrument and clocks are of the highest class, I had now all the means for inquiry which could be desired. I had already compared daily all the chronometers entrusted to my care, and recorded the daily extremes of temperature to which they were exposed. I now commenced a record of the sea-rate, when it was attainable, of every chronometer which passed through the observatory.

"From an inspection of these records, it appears, 1st, That no sensible change of rate seems to be produced, in ordinary cases, either by the motion of the ship or by magnetic attraction. My experiments are to a certain extent defective, as I have no good authority for the rates at foreign sea-ports; but I feel convinced that the general effect produced on the rates of chronometers, from the above causes, is so small as to be of little practical importance.*

"2dly, That well-made chronometers, *when new*, gain on their rates, excessively for the first few months (in some cases this gaining on the rate continues for two or three years), and gradually arrive at their fixed rate. There appears to be no chance of lasting good performance unless the balance-spring is made hard, and the tendency of new chronometers to gain on their rates seems to depend on this, for when the spring is soft they are not so affected. The inconvenience of this gaining on the rate is often very great (in some cases of serious consequence to the ship-master), and the probability of its occurrence in new or re-sprung chronometers cannot be too strongly impressed on his mind. It is not uncommon for new chronometers, which have been sent to sea too soon, to return in three or four months with an increased gaining rate of 8 or 10 seconds a-day; and the ship-master, not knowing the cause, not unfrequently loses all confidence in an excellent chronometer, which has thus, as he conceives, deceived him.

"3dly, When the balance-spring has been in use for many years, the elasticity becomes so much deteriorated as to render the performance very uncertain. Chronometers which have been going for 20 or 30 years with the same balance-spring, scarcely ever keep

* We shall probably soon have a most searching series of experiments on the going of chronometers at sea. Our Associate, Mr. Bond, is commissioned by the American Government to measure the arc of longitude between Boston and Liverpool *chronometrically*. Arrangements for the expedition are now in progress, and we look with great interest to the result, not merely as our astronomical link which connects Europe with America, but as likely to throw great light on more than one *vexata quæstio* in navigation.

a steady rate, however good their performance may have formerly been. With proper means for testing, this fault cannot escape detection; the defect shewn in the performance is similar to that of a new chronometer with a soft balance-spring: the rate, in either case, may be steady while the chronometer is kept in a temperature nearly uniform, but in a fluctuating temperature the rate will change in the most capricious manner. This defect in the performance may be removed by the application of a new balance-spring; after which the chronometer goes through the process of gaining on its rate exactly the same as a new chronometer. The elasticity of the balance-spring may be so much deteriorated that no dependence can be placed on the performance, when to all external appearance the spring is quite free from rust or injury. There are many chronometers in the merchant-service which from this fault are worthless, and which are nevertheless carried to sea voyage after voyage, and from year to year, although in most cases an expense of a few pounds in respringing would render them as efficient as ever.

"4thly, Between the periods at which the chronometer ceases to gain on its rate and that at which the elasticity of the balance-spring becomes deteriorated, very great confidence may be placed in the rates of a large majority of the chronometers employed in the merchant-service; the imperfect manner in which the ordinary balance acts may be traced with ease, and tables of corrections may be formed, depending on the temperature, which may be applied successfully. There is nothing in the foregoing statement which is not perfectly well known, I believe, to all experienced makers.

"I have been in the habit of giving with chronometers which have been sent to the observatory the rate which corresponds with various degrees of the thermometer; and if the chronometers are kept in a place not liable to rapid changes of heat, and the time is brought up each day by the rate assigned to the temperature noted, I feel satisfied that a large number of the time-keepers now made would give very satisfactory longitudes. But this mode of carrying on the time requires a peculiar rating, which, so far as I know, is only practised at this observatory, and demands, besides, more attention from seamen than they are usually inclined to give. Many attempts have been made to produce a more perfect compensation, and it is acknowledged by all skilful makers that this is at present the great desideratum in the improvement of chronometers.

"I have always considered it a part of my duty to give every facility to makers in testing their chronometers. Several makers in Liverpool have availed themselves of the advantage thus afforded to them by the observatory, and amongst others Mr. William Shepherd, of 13 Bath Street, Liverpool, sent his new chronometers to the observatory to be tested previous to their being sold. I was in the habit at this time of pointing out to those makers who took

an interest in the matter the defective state of the ordinary balance, as shewn by my experiments, and the ease with which the means at my disposal enabled me to detect any fault or improvement; and on doing this on one occasion to Mr. Shepherd, he remarked that he had tried many experiments on the balance, but had not been successful, and that if I could suggest any method which I conceived would remove the tendency to lose in extremes, he would make up the balances for me to try experiments with. I agreed to do so on condition that the results of our experiments, in the event of their being successful, should be made known for the benefit of the public, and to this Mr. Shepherd, without hesitation, consented. I had many discussions with Mr. Shepherd, and made several trials, from which I concluded,—

"1st. That the balance-rim must be of a circular form, so that the laminæ of brass and steel might be turned down to the requisite proportions with facility, and that the compensation and poising might be easily effected, as in the ordinary balance.

"2d. That the balance must be so contrived that the compensating rim and weights should move towards the centre with an accelerating velocity in an increasing temperature, while in a decreasing temperature they must recede from the centre with a gradually diminishing velocity.

"I need not mention those attempts which were either unsuccessful or which from difficulties of workmanship were not likely to be practically of use. I will describe that which is found to be easy of execution, and which is sensibly perfect in its operation.

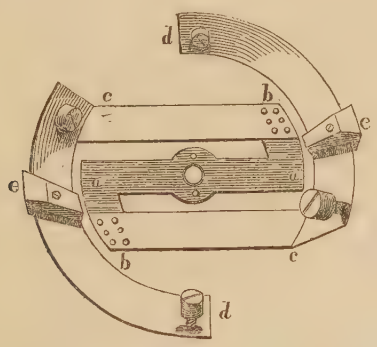

Fig. 1.

"In Fig. 1 the rim *c d*, *c d*, is composed of a strip of brass outside and a strip of steel inside, united as in the ordinary balance, and it differs from the rim of the ordinary balance only in its being *bevelled*, the angle being 45 degrees or thereabouts, as shewn in the elevation fig. 2. The bar *a a*, with the ears *a b*, *a b*, and also the bars *b c*, *b c*, are composed of brass and steel united,

Fig. 2.

as in the rim; *a a* has the brass uppermost, and *b c*, *b c*, have the steel uppermost, and they are firmly joined at *b*, *b*: *e*, *e* are compensating weights, which slide on the rim as in the ordinary balance. The screws shewn near *c*, *c* and *d*, *d* are timing and poising screws.

"Now the effect of the inclined position of the rim is this: the different expansion of the two component strips carries the weights *e*, *e*, to and from the centre, and also up or down in a *slanting* direction, owing to the bevelled form of the rim. The *slant* takes off from the action of the rim, but this is increased to the proper amount, at mean temperatures, by setting the weight more forward on the rim, so that the balance is compensated for small changes. So far the new construction acts exactly as the ordinary compensated balance.

"To shew how the new balance acts in extreme temperatures, first take the case of extreme heat. The ends of the bar *a a*, bend downwards, and the ends of the bars *c b*, *c b* curve upwards, and the compound effect of these two curvatures is to set the bevilled rim more nearly perpendicular to the plane of the balance, whence the effect of the compound rim in bringing the weights *e*, *e* towards the centre is greater than it would have been in its more inclined position. Now this tends to shorten the time of vibration of the balance. In the ordinary construction, a chronometer *loses* in extreme heat; the new construction, therefore, tends to compensate the losing rate in extreme heat.

"Again, in extreme cold, the ends of *a a* bend upwards, and the ends of *b c*, *b c*, bend downwards, from the unequal contraction of the brass and steel strips which compose the bar. The effect of this compound action is to set the rim flatter, or more nearly parallel with the plane of the balance. Hence the effect of the compound rim will be less effective in removing the weights *e*, *e* from the centre than in the ordinary construction, and the balance will move more quickly. A balance of the ordinary construction *loses* in extreme cold, hence the new balance tends to compensate the error.

"I find by my experiments that a chronometer furnished with the new balance is sufficiently compensated in all degrees of heat and cold, for a proof of which I refer to Table I. This contains the daily rates of the first three chronometers to which the above description of balance was applied from Jan. 16, to March 2, 1848, with the daily range of temperature to which they were exposed. The brass boxes containing the chronometers were suspended in gimbals, in the same way as chronometers are kept when at sea, during the whole time, thereby insuring uniformity of position under all changes of temperature.

"TABLE I.

Name of Chronometer, William Shepherd.

Date 1848.	222 Daily Gain. s	228 Daily Gain. s	230 Daily Gain. s	Temperature. Highest. °	Lowest. °
January 16	−1·0	−1·4	−2·4	85	70
17	−0·5	−1·3	−1·9	87	85
18	−0·7	0·1	−0·8	43	34
19	−1·9	−0·2	−1·0	35	31
20	−1·7	−0·6	−1·3	64	62
21	−0·5	−0·2	−1·3	65	62
22	−0·8	0·8	−1·2	36	33
23	−0·9	0·8	−0·7	34	31
24	−1·5	0·6	−1·1	37	31
25	−1·5	0·2	−0·2	38	33
26	−1·4	−1·2	−1·2	36	32
27	−1·4	−0·9	−0·7	73	70
28	−0·2	−0·9	−2·2	103	95
29	−0·4	−0·7	−1·7	26	20
30	−0·8	−1·1	−0·3	40	28
31	−0·1	−0·8	−0·3	43	34
February 1	−0·9	−1·7	−1·4	75	68
2	0·6	−0·8	−1·2	103	93
3	1·0	−0·2	−2·4	105	93
4	1·3	−0·1	−0·9	50	48
5	0·8	−0·5	−1·6	53	49
6	1·1	0·0	−1·5	60	53
7	0·8	−0·2	−1·5	80	73
8	0·8	−0·1	−1·6	103	93
9	1·2	0·4	−1·7	93	85
10	1·3	0·3	−0·8	70	65
11	1·3	0·5	−1·0	100	91
12	0·8	0·5	−2·1	95	87
13	1·0	0·3	−1·1	62	56
14	1·0	0·2	*	54	51
15	0·6	−0·1	1·0	55	52
16	0·4	−0·1	0·6	90	85
17	0·8	0·2	0·0	98	90
18	0·7	−0·1	0·2	38	31
19	0·1	−0·2	0·2	45	39
20	0·4	−0·4	1·2	55	50
21	0·6	0·1	1·6	70	65
22	0·8	0·6	1·2	98	91
23	0·8	0·7	0·8	90	85
24	1·0	0·8	0·8	90	88
25	1·2	0·9	1·0	97	92
26	1·6	0·3	1·7	58	54
17	1·7	0·6	2·1	57	53
28	1·9	0·8	2·3	57	53
29	1·6	0·7	Rate closed.	59	53
March 1	1·4	0·4		55	51
2	1·4	0·5	0·4	55	50

* "The weights were moved a little further from the arm, as the chronometer appeared to lose more in heat than in cold."

"For extreme cold the chronometers were exposed to the open air on the north side of the building; for all other temperatures they were either placed in the chronometer-room or in the heating apparatus. This apparatus is heated with hot air produced by the burning of gas, the pressure of which is regulated by a *governor*, and any definite temperature from that of the room to upwards of one hundred degrees may be kept for any required time, with a variation not exceeding two or three degrees in twenty-four hours. The artificial heat thus applied can do no more injury to chronometers than would heat arising from natural causes, and the removal of the wooden box in which the chronometer is suspended in its gimbals is rendered quite unnecessary, since heat applied in this way has little tendency to split or warp the wood.

I have given the daily rates in Table I. that they may be compared with the daily rates of chronometers with the ordinary balance, through the same range of temperature; but with the means possessed at the Liverpool Observatory, the power of the balance to compensate perfectly through a long range of temperature may be tested much more expeditiously, and the experiments repeated as often as desirable. I will give an example of the method of testing which I have practised, and which shewed, in a few hours' trial, the defect in all the constructions which were devised by me, and made by Mr. Shepherd, previous to that which I have described.*

"In my first experiments with the new balance I had ascertained what I had not been able to make out with the ordinary balance,—namely, that by a sudden change in the temperature of 20° or 30°, the balance-spring was affected sooner than the balance, and that a sudden immersion into a temperature higher by 20° or 30° caused the chronometer to lose, during the first hour, two or three tenths of a second more than it afterwards did by continued exposure to the same temperature. By sudden immersion into a lower temperature of 20° or 30° the same variation took place, but with a contrary sign, and the chronometer gained two or three tenths of a second more during the first hour than afterwards. But by changing the temperature *gradually*, and at the rate of not more than 8° or 10° an hour, the balance appears to be sensibly affected as soon as the spring.

"To shew the difference between the ordinary and the improved balance, I subjoin Table II., which contains the rate of the same chronometer with balances on the two constructions. Col. 1 is the interval in decimals of a day, at the beginning and end of which the chronometer was compared by coincident beats with a sidereal chronometer, and the highest and lowest temperatures noted.

* "By a test similar to that in Table II. an improvement was shewn in the very first balance with the dished rim, and by the application of the compound centre bar it was rendered so perfect that I have not yet been able, notwithstanding the numerous experiments which I have since tried on twelve consecutive chronometers of this construction, to detect any sensible tendency either to lose or gain in extremes."

Col. 2 is the deduced daily rate. Cols. 3 and 4, the temperatures. These are in the order of time. At the side are two other columns, in which the daily rates are arranged in order of temperature.

"TABLE II.

William Shepherd, 222. *Ordinary* Balance.

In order of Time.				In order of Temperature.	
Days.	Daily Gain.	Temperature. Highest.	Lowest.	Daily Gain.	Mean Temperature.
	s	°	°	s	°
0·242	−5·8	47	37	−9·0	34·5
0·683	−0·7	82	65	−5·8	42·0
1·084	−2·4	102	82	−3·7	50·5
0·064	−4·1	108	102	−1·0	65·0
0·684	−5·8	113	108	−0·7	73·5
0·542	−5·6	113	102	−1·7	83·5
1·180	−1·7	102	65	−2·4	92·0
0·767	−1·0	65	65	−4·1	105·0
0·685	−3·7	65	36	−5·6	107·5
1·001	−9·0	36	33	−5·8	110·5

William Shepherd, 222. *Improved* Balance.

In order of Time.				In order of Temperature.	
Days.	Daily Gain.	Temperature. Highest.	Lowest.	Daily Gain.	Mean Temperature.
	s	°	°	s	°
0·375	−2·9	34	33	−2·5	28·5
0·500	−2·4	33	32	−2·4	32·5
0·127	−3·2	33	32	−3·2	32·5
0·980	−3·4	73	33	−2·9	33·5
0·249	−3·6	103	73	−2·9	37·0
0·753	−1·9	103	95	−3·2	51·5
1·000	−2·5	35	22	−3·4	53·0
1·002	−2·9	40	34	−2·4	71·5
0·125	−3·2	68	35	−3·6	88·0
0·875	−2·4	75	68	−2·0	88·5
0·250	−2·0	102	75	−1·9	99·0

"Between the trials with the old and new balances, the chronometer was not cleaned. Nothing whatever was done to it, except the detaching of the old balance from the spring, securing the new one to it, and adjusting the weights of the new balance, so as to effect the compensation. The variation of rate due to extreme temperature in this chronometer *with the ordinary balance* is about the quantity which I have usually found in good chronometers from a similar change of temperature.*

* "Immediately subsequent to these experiments, I requested Mr. Shepherd to allow me to ask to have the same chronometers tested at the Royal Observatory. Not that I had the least doubt on my own mind as to the efficiency of the balance, but because I thought the principle would be sooner adopted by chronometer-makers, on a successful report appearing from Greenwich, than by any other means which I could adopt. The Astronomer Royal, on my application to him, very kindly obtained permission to have this done, and the rates of the three chronometers in Table I. are published in the list of trials for 1848.

"Selecting from the Greenwich rates the two weeks of highest and lowest

"The disadvantage of the new balance is, so far as I know, merely in the additional trouble of construction. Mr. Shepherd thinks that it requires at least twice the labour of the ordinary balance. The

temperature, and that which corresponds the nearest to the mean between them, we have the following rates:—

"*William Shepherd.*

	222	228	230	Temperature.	
	Daily Gain.	Daily Gain.	Daily Gain.	Max.	Min.
	s	s	s	°	°
March 11 to 18	−2·7	−1·1	−0·5	46	40
April 1 to 8	+1·0	+0·4	−1·2	71	63
May 6 to 13	+1·4	+0·5	−3·1	110	85

"Selecting again another set in a similar way, in which the temperatures are not very widely different, we have

	222	228	230	Temperature.	
	Daily Gain.	Daily Gain.	Daily Gain.	Max.	Min.
	s	s	s	°	°
April 22 to 29	+0·9	−0·3	−2·2	58	46
July 1 to 8	+0·2	+1·1	−0·5	77	57
June 17 to 24	+1·6	+2·0	−2·4	107	82

"Now, by comparing the two sets, it will be seen that the rates differ from each other in similar temperatures, nearly as much as they differ from each other, in each set, in different temperatures; and it appears to me that the law of variation between the balance and spring cannot be deduced from such discordant results.

"The trial does, however, shew that the chronometers did not go so steadily at Greenwich as at Liverpool; and I can account for this in no other way than by supposing that it arose from the chronometers being taken out of their gimbals, which I now understand was done at Greenwich.

"I understand from Mr. Shepherd, that No. 228 was purchased by the Government; Nos. 222 and 230 were again tested by me subsequent to the Greenwich trial. In the wooden boxes mounted in gimbals the rates were as steady as those given in Table I. When out of the gimbals, and standing on the table, a projecting pin at the bottom of the brass box inclined the face of each chronometer at an angle of about 4°, more or less, depending on which side of the face was elevated, the pin not being in the centre of the bottom of the box. In this state the rates were quite as discordant as the Greenwich rates, owing, as I at the time supposed, to the inclined position of the chronometer; but I now think that part of the irregularity arose from the unsteady way in which the box was supported: resting, as it did, on the end of the pin and the rounded part of the bottom of the brass box, the vibrations of the balance might cause the chronometer to attain an oscillatory motion. I understand from Mr. Shepherd, that the chronometers had only been poised in the callipers.

"There is no difficulty in making chronometers go at nearly the same rate with the 3, 6, 9, and 12 upwards; but when this is done, box chronometers generally gain several seconds a-day more with the face upwards than in any of the above-named positions. I am not aware of any method of getting rid of this fault, except that of making the chronometer fast in the short vibrations; but the remedy is certainly worse than the disease.

"So far as my experience goes, an error in positions of 10^s or 15^s is of little practical importance in chronometers employed for nautical purposes. I have tested a great many timekeepers of different makers, and have frequently found chronometers, which have been celebrated for their steady rates when free in their gimbals, upwards of half a minute a-day out in positions.

"This is, however, a question which has no bearing on the balance which I have described, since that may be poised in positions with as much facility as the ordinary balance."

advantage is, that when made to compensate for any two distant temperatures, such, for instance, as 60° and 90°, or for 30° and 60°, I have found the compensation equally perfect in the lowest temperature which I have been able to obtain, or in the highest that I could risk the chronometer in, and sensibly the same in all intermediate temperatures. I have subjected chronometers with this description of balance to temperatures as high as 124° and as low as 20°, and whenever a loss has been found at one extreme, a tendency to gain at the other extreme has invariably presented itself. Under these circumstances, and as I understand from Mr. Shepherd that he applies this description of balance to all the chronometers which he now makes, pocket as well as box, I am anxious to make it known that chronometer-makers and the public may avail themselves of it if they think fit."*

Description of a small Transit-room, with Section and Plan.
By Mr. Dell.

The dimensions of this room are $7\frac{1}{2}$ feet in the meridian, $5\frac{1}{2}$ feet wide, and $6\frac{1}{2}$ feet high. By placing the pier and window-slits a good deal on one side, Mr. Dell finds he has room enough for the clock and a desk.

The instrument,—a 30-inch transit by Jones,—stands on a stout stone, resting on a brick pier, which descends $2\frac{1}{2}$ feet below the ground. The building itself is of wood, which, being screwed together, can be taken apart and set up again in a very short time, if necessary. The cost, including labour and materials, did not exceed *6l.*

Mr. Dell is well satisfied with the steadiness of the instrument.

Before placing the transit in the meridian, Mr. Dell observed *η Ursæ Majoris* and *γ Draconis* on the prime vertical, and deduced, as a mean result, Lat. 51° 48′ 54″·7 N.

The observatory was found, by measurement, to be 522 feet south and 295·5 feet east of Aylesbury spire, or 5″·1 and $0^s{\cdot}31$ respectively. Adopting the data of the survey, viz.—

N. Lat. = 51° 49′ 1″·0 and $3^m\ 17^s{\cdot}07$ W.,†

Mr. Dell's observatory is in

Lat. 51° 48′ 55″·9 N. and Long. $3^m\ 16{\cdot}76$ W.

Mr. Dell also finds,—

Hartwell Observatory	4679 feet South,	6842·8 North of	Aylesbury Spire
Stone Observatory	6577 ——	11330·0 ——	

The Rev. J. B. Reade recommends that the eyehole of the ordinary negative, or Huyghenian, eyepiece should be much reduced

* "I hope shortly to have an opportunity of testing this balance in the extreme temperatures of Cambridge, U.S."

† Is this the longitude of the published survey, in which an erroneous figure of the earth was used, or a corrected longitude? The difference is about $0^s{\cdot}8$.

in size, when the telescope is used for viewing the sun. He finds that, with a small eyehole, such as is necessary in the Gregorian telescope, the whole aperture of the object-glass may be used without endangering the dark shade.* The eyehole should be placed where an image of the object-glass is formed by the eyepiece, or, what is the same thing, where the axes of the different pencils cross each other. The aperture of the hole should not be less than the image formed by the object-glass, and need not be larger. Mr. Reade finds, from actual experience, that this position and limitation of the eyehole greatly improves the definition of the telescope for all celestial objects.

Extract of a Letter from Captain Hardy to Dr. Lee.

"I wish to call your attention to four remarkable spots, all belonging to the same mass, which are now (May 8) passing across the sun's disc. They entered his upper limb on the 29th or 30th of April, a little to the eastward of his north pole. They proceeded southward in nearly a straight line, and the principal spot crossed his equator about May 4. They are now approaching the lower limb, and are, at $3^h\ 30^m$ this day, two-thirds of the way down. The form of this mass is singular, and in many respects different from anything I have seen before."

A series of drawings of the sun has been forwarded from Poona by Capt. Jacob. Capt. Jacob, we are informed, has been appointed to succeed the late Mr. Taylor at the Observatory of Madras.†

Lunar Theory.

It will be remembered that in the investigation of the correction of the Elements of the Moon's Orbit required by the Greenwich Lunar Observations, the Astronomer Royal pointed out the following term as not recognised by theory but of undoubted existence :

Inequality in the Moon's Latitude $= +2''{\cdot}17 \cos u$;

* Mr. Reade is of opinion that the thermal rays, in a refracting telescope, do not follow the same course as the illuminating rays; that they are considerably diffused at the eyehole, and that a large portion is stopped there when the eyehole is small. Have any experiments been made to ascertain the path of thermal rays when transmitted through an achromatic object-glass and through different kinds of glass?

† The Editor would suggest to the observers of solar spots the necessity of being very careful and specific in their *measures*. No safe conclusion can be drawn from general impressions or vague descriptions. The situations of the spots should be referred distinctly and by measure to the limbs or centre.

A guess has been hazarded that the spots are not attached to the sun, but are rather of the nature of planets or comets revolving round him. It need scarcely be stated here that there is no probability in favour of this supposition, which was exploded by Galileo on satisfactory grounds. Besides his reasons, the spots cannot be *free* bodies *near* the sun, their motions are too slow; while the absence of parallax proves that the spots cannot be near the earth, the only position which they *could* occupy to satisfy the slow motion. Every intelligible account of the solar spots seems to confirm Sir William Herschel's hypothesis, that they are variable openings in the luminous atmosphere of the sun.

where u is the moon's true longitude. (See *Monthly Notices*, 1848, June 9, vol. viii., No. 8, page 187.)

We have the gratification now to state that this term has been shewn to be a legitimate deduction from accurate theory. Our Associate, Professor Hansen, of Gotha, in a letter to the Astronomer Royal, has given the following account of it: — It is known by elementary investigations in physical astronomy that, supposing the plane of the earth's orbit to be invariable, the mean inclination of the moon's orbit to the earth's orbit is constant. Laplace made a very important addition to this theory in shewing that, though the plane of the earth's orbit is undergoing a change from the action of the planets, steadily progressing in one direction, yet the mean inclination of the moon's orbit to the variable plane of the earth's orbit will be sensibly constant. Now, Professor Hansen has introduced a slight modification in this theory. He finds that the position of the moon's orbit will be more correctly represented by the following conception. "Take the plane of the earth's orbit in the position in which it was about three years ago; let the nodes of this plane revolve backwards on the present ecliptic through 90°, without change of inclination, the plane so found is the plane to which the uniformity of inclination of the moon's orbit is to be referred." The effect of this, omitting terms which are inseparably mingled with other terms, is to produce in the moon's latitude the inequality,—

$$= +1''\cdot38 \cos \text{moon's longitude};$$

that found from the observations,—

$$= +2''\cdot17 \cos \text{moon's longitude.}$$

The same train of investigation has given a term in the moon's longitude,—

$$= -0''\cdot50 \cos \text{longitude of node};$$

while that deduced from the observations, and hitherto unexplained, is,—

$$= -0''\cdot97 \cos \text{longitude of node.}$$

The small differences which remain are in all probability due to errors of observation."

After the termination of the sitting, the Astronomer Royal exhibited a specimen which he had received from Mr. May, illustrative of the construction of the pivots of the new transit circle for the Royal Observatory, of which the massive parts are now progressing under the superintendence of Mr. May. By long meditation on the causes of uncertainty in instrumental observations, and more particularly by consideration of the state of the Cape Mural Circle, in which (after its return to England), it was found that its steel axis-collar had been merely fastened on by soft solder, and could easily be turned by hand, the Astronomer Royal had been led to the determination that, in every new instrument constructed under his direction, the pivots should be cast in the same flow of metal with the parts which they immediately support. The advantages which cast-iron presents for the construction of large instru-

ments (from the facility with which it is cast, and its small thermal expansion when in the manufactured state), had long made him desirous of using that material. And Mr. May had pointed out to him that these objects might be obtained, giving at the same time extreme hardness to the surface of the pivot, by the process called *chilling*, in which the pivot, or other part to be hardened, is cast in a mould of iron at a certain temperature, other parts (in the same flow of metal) being cast in sand in the usual manner. The chilled surface is too hard to be turned on the lathe by a tool in the ordinary way, but it may be turned with most perfect accuracy, and most complete command of its surface, by using, instead of an ordinary tool, a leaden wheel charged with emery, which is turned with great rapidity, and is at the same time moved backwards and forwards in the direction of the axis of the pivot: the pivot being turned slowly to it as in the ordinary lathe-turning.

The specimen forwarded by Mr. May was a part of a perforated cylinder, in all respects similar to the instrument-pivots, which had been chilled in casting, had been turned and ground in the manner above described, and had been broken, so as to shew the depth to which the chilled structure penetrates.

It has already been mentioned that the splendid heliometer by Repsold (the largest and most perfect instrument, we believe, now existing of its class) has been received at the Radcliffe Observatory quite safe. The Trustees have directed that it should be mounted with corresponding magnificence, and the building is now nearly completed. A handsome round stone tower, in advance of the east wing of the observatory, is carried up high enough to give the instrument a clear view to the north horizon; the south is open, and the only direction in which the view is confined is to the north-west, where the central tower of the observatory intercepts an unimportant part of the sky. We are confident that in Mr. Johnson's hands the instrument will do credit to the talents of the artist, and justify the discerning liberality of the Radcliffe Trustees.

Books, papers, &c. for Professor Schumacher may be forwarded to Mr. Dent, who will transmit them to Altona as occasions offer.

Mr. Beaumont wishes to dispose of a transit telescope, $2\frac{3}{4}$ inches' aperture, of the best construction, with its Y's, near collimating mark and lens, morocco observing chair, and copper shutters, for 80*l.* The original cost was above 200*l.*

Also of a clock by Molyneux.

ERRATA.

In Mr. Airy's Circular of May 21, in the epoch *for* "March," *read* "May."

P. 139, Captain Shea *presented* his book of drawings.

P. 143, line 28, *for* "L +," *read* "L −"

,, ,, *for* "L′ +," *read* "L′ −"

but it is safer to work out each case according to its nature, and not to follow a formula.

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